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THE OBSERVATORY,

A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

E. W. MAUNDER, F.R.A.S.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 81. JANUARY 1. 1884.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1883, December 14.

E. J. Stone, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. Glaisher, M.A., F.R.S., and E. B. Knobel.

The Minutes of the last Meeting were read and confirmed.

Mr. Knobel announced that 67 presents had been received since the last Meeting. Amongst these was a complete series of the observations of solar spots, of double stars, and of the zodiacal light made at Rome by Padre Ferrari; also two old Sun-dials, evidently made in Paris in the last century.

The thanks of the Meeting were unanimously voted to the respective donors.

On the motion of *Mr. Finch*, seconded by *Mr. Mattieu Williams*, Messrs. Lecky, Talmage, and Tupman were elected Auditors.

Mr. Ranyard read a paper "On a Narrow Belt on the Planet Saturn." He believed narrow belts were very rare on Saturn, and did not know of any others having been observed. On the 4th November, with an 18-inch glass reflector, he observed a narrow belt stretching across the disk and slightly fading away towards either limb. It was nearly as easily seen as the Cassini division on the ring. He did not determine its position very accurately. He showed it to two persons, who also saw it. He saw it again on the 13th November, but not so clearly. He nevertheless felt quite sure of it, and watched it for two hours, and at moments, when the definition was good, traced it across the planet. On the 21st he saw it again, and showed it to Mr. Hopkins. He had tried to find whether any other person had seen it, and found that Dr. Copeland, on the 6th November, had seen a belt which was probably the same.

Mr. Knobel. Mr. Ranyard has drawn the belt as a straight line. Would not that be an anomalous position, seeing that the axis of the planet is at present much inclined towards the Earth?

Mr. Ranyard. I think I have given it a slight upward curvature; perhaps I ought to have given it more. All that I am sure of is the position of the middle of the belt; it was decidedly lower down than the centre of the planet. There was a slight fading of the belt at the limb, but probably it was not so marked as in the case of Jupiter.

Mr. Knobel. Did it appear quite as straight a line as you show it?

Mr. Ranyard. No; it was more curved, though not as deeply curved as you suggest. I do not think that the present inclination of the axis (less than 26°) would make the belt appear as a semicircle. I have given it some degree of curvature downward, but probably not enough.

Mr. De La Rue. If Mr. Ranyard did not trace the marking nearly up to the limb, he might easily be a little misled. If the belt in the drawing were continued to the limb, the true curvature would be more apparent.

The President. A good deal depends on the point to which Mr. Ranyard's attention was attracted.

Mr. Ranyard. My attention was chiefly directed to the definition, and not to the curvature of the belt. A good deal of time was occupied in changing eyepieces, and asking people to look through the telescope as witnesses to what I believed I saw.

Major Herschel. The question is, whether Mr. Ranyard noticed any thing anomalous with regard to its proper form in that parallel of latitude.

Mr. Ranyard. My attention was not attracted by any thing anomalous.

Mr. De la Rue. I would suggest that Mr. Ranyard should make another drawing, giving the projection consistently with the position of the axis of Saturn, and then say whether that represents what he actually saw.

Mr. Ranyard. In Mr. Hopkins's drawing and in Dr. Copeland's description the belt is given as a little further towards the south than in mine. I make it a little nearer the equator.

The President described a paper "On the supposed possible independence of Bessel's and Leverrier's expressions for the mean Longitude of the Sun, subject to the conditions imposed upon them by their use in Practical Astronomy in the determination of mean Solar Time." He said, Professor Cayley and myself are quite agreed that if you consider Bessel's expressions and Leverrier's expressions for the longitudes of the mean Sun to refer to two perfectly independent moving points, then my result would be wrong and the old view correct; but that if these expressions must represent any continuous motion as I maintain, then that my result is right. I have therefore, in the present paper, tried to show that as we use these expressions they are of necessity the representatives of a real motion, viz. that of the Sun itself in mean longitude. For before we can compare any lunar theory with observations, we require not only the theoretical position of the centre of gravity of the Moon, but the centre of gravity of the Earth, and the motion of the Earth about its centre of gravity. And as we make no distinction in our theories between the real motion of the Sun in longitude and that of the mean Sun, the meaning of the real Sun is fixed when it is used to determine the mean solar time from sidereal observations; and the value of the Moon's mean motion thus becomes absolutely dependent upon the particular value adopted for the mean motion of the mean Sun.

The real difference between my views and those of Professor Adams appears to arise from Professor Adams considering that in some way or other we have a measure of the true mean solar day, defined and fixed by authority, like a standard yard; whilst I cannot recognize that such a unit is available in practice, and consider that we have to proceed from what is known, and assign it as some definite angle, which must be the angular velocity of the Sun in some interval of time, which I adopt in consequence as my unit. The exact proportion of this adopted unit to the true mean solar day would not be known until our theories were perfected; but as all changes in the adopted angles are necessarily proportional to those in the adopted units, we can in this way always correctly allow for changes. I have very carefully considered my views, and I am satisfied that the results which I have brought forward are correct.

Professor Adams. I am sorry to differ so completely from Mr. Stone. I think a great deal of the matter he has introduced is irrelevant. The subject of the determination of the mean solar time appears to me to be a very simple matter. The President has said that the mean Sun is an arbitrary body. The mean Sun is simply the place of the Sun cleared of the ecliptic and other periodical inequalities to which the Sun's motion is subject.

Professor Adams then read a paper which he had prepared on this subject. The mean Sun differs, he said, from the true Sun by a calculated quantity which was positive or negative. The mean Sun is quite distinct from an arbitrary Sun. Mean time is simply determined by the transit of that body: supposing it to move in the equator instead of the ecliptic, mean time is measured by the motion of that body in hour-angle from the meridian of a particular place, and not by the motion of that body with reference to the equinox. In our measure of time, mean time increases by 360°. Whilst the mean Sun leaves the meridian and returns to it again, in a year the mean Sun performs 365 revolutions, and not merely one; whereas, if you refer to the motion of the Sun with regard to the equinox, it only performs one revolution in the course of the year; therefore the actual rate of revolution by which we count time in regard to a fixed meridian is 365 times as rapid as the motion in longitude, which is the whole matter. The motion of the mean Sun in longitude is what defines time according to the President. The discrepancy between Mr. Stone's theory and the fact arises from this, that mean solar time is not measured by the motion of the mean Sun in longitude, but by its motion in hour-angle, which is 365 times greater in amount. Mr. Stone's theory does too much; it over-corrects, and as years go it will over-correct more and more.

The President, in replying, wrote the following equations on the blackboard, and asked had Professor Adams considered the effect upon his calculations of the term $2\pi\frac{\delta n}{n}.t$, which has usually been neglected.

Sine $s=A+(2\pi+n)t_1$=Bessel's expression, is the expression for the sidereal time; and if $t_1=t\left(\frac{n+\delta n}{n}\right)+a$,

we get $$s=A+(2\pi+n+\delta n)t+2\pi\frac{\delta n}{n}.t+(2\pi+n)a,$$

which can be made equal to

$$=A_1+(2\pi+n+\delta n)t+2\pi\frac{\delta n}{n}.t$$

$$=\text{Leverrier's }+2\pi\frac{\delta n}{n}.t.$$

Mr. Glaisher said he had a paper, communicated by the *Astronomer Royal*, "On the Spectrum of Comet *b*, 1883;" and another paper "Observations of Comet *b*, 1883 (Pons-Brooks), made at the Royal Observatory, Greenwich."

The Astronomer Royal said, in regard to the first paper, that the spectrum of the comet was the usual cometary spectrum, and consisted of three bands agreeing in position with the principal bands seen in the spectrum of the Bunsen flame. There did not appear to be any very decided continuous spectrum except from the nucleus and the parts of the head immediately around it. The brightest of the three bands, that in the green, was traced for a length of 4′ or more. The observations had been made by Mr. Maunder.

Mr. Glaisher said two or three papers had been received referring to the subject Mr. Stone had brought forward. One was from *Major-General Tennant*, "On the change of the adopted Unit of Time;" and from *Professor Cayley*, "On the change of the Unit of Time implied in the substitution of Hansen's for Bessel's expression for the Longitude of the Mean Sun;" and a third, from *Mr. Stone* himself, "On the supposed unreliability of the Greenwich Observations of the Sun in mean results."

Mr. Mattieu Williams said that it had been suggested to him that the remarkable sunsets we had witnessed of late were due to a cloud of meteoric dust. He had therefore exposed slips of glass (ordinary microscopic slides) in suitable situations where dust ordinarily fell, and then touching them with a little acid, and adding the ordinary reagents, had obtained an indication of iron. About the 5th or 6th Dec. a snow-storm came on, and the wind changed. It was N.E. previously to the experiments being made, and then it changed to the N.W. The experiments were made at Stonebridge, near the edge of the Weald of Harrow, and with the wind in that direction none of the atmosphere of London was brought. The snow came across a part of England less populated than any except the Moors, so that the sample was as good as could have been obtained at a place a great many miles from London. He had collected as much as 75 ounces of the snow, and upon testing it with hydrochloric acid had obtained the usual indications of iron. One interesting feature about the precipitate was that it was more of a green colour than is obtained from pure iron. He had not had time to make a quantitative analysis, or to go into the question as to whether it contained nickel; but the presence of nickel was strongly suggested. He had obtained indications of the presence of magnetic oxide of iron, which had been confirmed by the magnet, the larger granules of the deposit being those that showed the most decided magnetic property. Of course this afforded no positive evidence of any thing abnormal, because at present there were no data to show what was the normal condition of the deposit that usually comes down with the snow; and until the presence of nickel was decided positively, the result was not so satisfactory as it might be. His principal object in bringing the matter before this Society had been to suggest that observations of this character should be made regularly at such observations as those of Ben Nevis and the Puy-de-Dôme, and that calculations should be made of the fluctuations of the percentage of iron, and possibly of nickel, that may be found in the snow at different places under varying meteorological conditions. Such observations could scarcely fail to bring out some interesting facts. It was necessary first to ascertain the normal amount of iron present, and whether that amount varied with the direction of the wind or other conditions or at different periods of the year, and whether the normal deposit was like the meteoric dust suggested in the present case, and whether this was the normal amount. Of course a great many precautions would be necessary; but he hoped we should have in future many observations in many places, and more particularly at those observatories in elevated positions, and perhaps the cause of these sunset effects might be brought out.

Mr. Knobel. I should like to ask why Mr. Williams attaches importance to the presence of nickel. He did not mention why nickel should be suggested. He said there was a green colour about the precipitate, and that that was evidence which indicated nickel.

Mr. Williams. The importance of nickel is this—meteoric iron is usually associated with nickel, whereas terrestrial iron is not.

Mr. Knobel. Yes; but is there no further evidence of the presence of nickel than a slight greenish colour in the ferro-cyanide of iron?

Mr. Williams. I have gone a little further than that; I might mention another indication. I have tested a small quantity of the filtrate from the cyanide. Cyanide of potassium in excess absorbs cyanide of nickel, and it is soluble in excess. I found in that filtrate indications of something very suspiciously like nickel. But though nickel is suggested by the experiment, its presence is by no means proved, and I must apologize for bringing the subject forward in this imperfect manner.

Mr. Ranyard. Mr. Williams sent me a little of this snow-dust, and it seems to be satisfactorily free from terrestrial matter. There was no indication of carbonate of lime, as there would have been if it had contained dust from the Weald of Harrow. It contains a great number of particles which I have been used to call meteoric particles; they are covered with black oxide, and are of peculiar shapes, usually very small. There are some larger particles of an amorphous kind, some covered with red oxide, others with black oxide, and there are grains of quartz and sand. I have compared it with volcanic dust produced by an eruption at St. Vincent that also contains some black oxide. It is possible the heat of the volcano may be sufficient to convert iron into the black oxide; so it is possible that we may have magnetic particles in the upper air that are not meteoric. They would be proved to be meteoric if they contained nickel in any large quantity. If some of the dust is immersed in glycerine on a microscopic slide, and a bar magnet is brought into the neighbourhood, the particles attracted by the magnet will be seen to turn as you carry the magnet round, and you can immediately see which are magnetic. The small meteoric particles are of a peculiar shape, so that after a little practice it is possible to tell which will turn with the magnet as you watch them under the microscope.

Mr. Knobel. You mean that they possess the property of polarity.

Mr. Ranyard. Yes. These particles have been tested with sulphate of copper, and it is found that metallic copper can be thrown down on them, and that shows that they contain metallic iron. Mr. Williams's dust was darker than that I have found in snow in former years, in which there was a comparatively light powder, like some I have here (volcanic dust), which is nearly white. Mr. Williams's specimen is quite black. It is an interesting question whether this matches the colour of the dust thrown up in the Java region, and whether it came from that volcano. Can we account for dust thrown up into the higher atmosphere appearing all over the world at once? For example, the eruption took place on the 26th of August. On the 2nd Sept., a blue sun and sunset phenomena were seen in the West Indies. Could that have been produced by the volcanic eruption? If so, we must believe that the storm would travel round the earth at the rate of 2000 miles a day for seven days, without producing a cyclone at the sea-level. There was a cyclone in the West Indies three days after (on the 6th September), but there were no great storms elsewhere. We can hardly predict what is possible in nature, but it does seem to be improbable at first sight; a good deal of evidence is needed to prove that that dust was brought from the volcanic region of Java. The sunset phenomena were seen in other directions at the same time. In Australia they were seen on the 10th Sept., and possibly earlier, on the 9th in Ceylon, and on the 2nd on the Gold Coast. The cause must have been widely spread and very rapidly spread. If it was a meteoric cloud that the Earth passed through, one would expect a whole hemisphere of the Earth to be equally covered with it. Light will be thrown on the subject if we can find out when the phenomena began at the various stations. I think facts are coming to light which show that it was not equally distributed over a whole hemisphere at first; but if dust from the Java eruption can be collected, and dust from snow, then the two dusts can be compared to see if they match. I may mention that this dust brought down in the recent snow contains a number of rounded particles, quite as much rounded as the Egyptian dust that has been blown from the Sahara. The angles are very much rounded. It also contains a quantity of black oxide; and it seems to be intermediate between the Sahara dust and the smaller particles of dust blown by the Sirocco winds and deposited at Rome and Naples, which is, no doubt, the finer portions of the dust carried up from Africa.

The Astronomer Royal. It is an important point to settle whether the black dust has its origin outside the Earth, or whether it may be due to local terrestrial causes. I should therefore like to ask Mr. Williams whether there is any possibility of the iron in this dust coming from any of the furnaces in England. He mentioned that the wind was from the north-west; and that suggests a suspicion. For that reason I think his proposal that observations should be made on the tops of mountains a very good one. I should also like to ask whether these particles showed repulsion under the application of the magnet as well as attraction, because if they were truly magnetic, they would show repulsion at one pole, and attraction at the other.

Mr. Knobel. Mr. Ranyard said they exhibited polarity.

The Astronomer Royal. They were repelled?

Mr. Ranyard. Yes; they also exhibited polarity, and formed themselves into little strings, with the bar magnet. But black magnetic oxide from the furnaces would also show that.

Mr. Williams. There is nothing definite shown from that. If you have a piece of soft iron it does not show polarity; but when it is sufficiently hard it assumes polar disturbance, and you get repulsion as well as attraction, and then you would get the rotating disturbance described by Mr. Ranyard, but not repulsion. I do not think under any circumstances you would get repulsion of small particles strongly magnetic.

The Astronomer Royal. What I want to know is, if there is any distinct evidence of the iron being truly magnetic—not merely of the particles being iron; because the magnet will attract iron, but it is only magnetic oxide of iron that exhibits attraction and repulsion. I understand that it requires a little delicacy to determine that with such small particles.

Mr. Williams. I used the term magnetic oxide generally. We call it magnetic because it exhibits a magnetism similar to the iron.

The Astronomer Royal. I merely wished to have a fuller explanation of what was meant by the dancing about under the action of the magnet.

Mr. Williams. Then, as regards the travelling of the iron-dust from the furnaces, the nearest furnace of any magnitude would be about 100 miles away. The specific gravity of these granules would be about $4\frac{1}{2}$ times the specific gravity of water, and they would not range through the air very rapidly. They would be as large as a pin's head.

The Astronomer Royal. Were not the furnaces in the direction of the wind?

Mr. Williams. Some would be (in Northampton and South Staffordshire), but this was close to London.

The Astronomer Rogal. It seems possible that they might come as far as that.

Mr. Williams. Some of them would not go two miles.

Mr. Brett. Are not these particles supposed by some people to come from Java?

Mr. Lecky. There was a letter on this subject from Mr. Preece in yesterday's 'Times,' explanatory of the enormous height to which volcanic dust in an electric state could be raised; and there is no doubt that in the desert the sand is carried up by electric currents to a very great height. There is a description of this in a book published, about 25 years ago, by Mr. Atkinson, on the 'Upper Amoor;' and he describes the sand-storms in the desert of Gobi, and gives a description of pillars of sand moving over the face of the desert. There is also Capt. Butler's 'Great Lone Land,' in which he describes enormous currents of electricity passing from the earth to the atmosphere. Mr. Preece refers in his letter to this volcanic dust, which is carried by such enormous electric currents into the atmosphere, where they would be kept up by the repulsion of the electricity. I would strongly recommend every one to read Mr. Preece's letter.

Mr. Ranyard. I do not think it is necessary to assume that there is any electric action about the pillars of sand. I observed them when I was in Egypt with the Eclipse Expedition. There was a great stretch of sand, some 9 miles in length, on the opposite side of the Nile, and every morning when the wind blew we were able to see it carry away a tree of sand. We could watch it through the telescope very well, and I did so several times and saw the tree form. It is mysterious how this dust, if it comes from Java, should remain in the air more than three months in such large particles. I cannot see myself how it is they did not come down. If they are meteoric dust, they would be in the upper atmosphere and might go up and down as spiders' webs do, carried up in the daytime in the hot air, and coming down at night, so that there would be no direct fall. The Sahara dust does not remain in the air so long, though it occasionally gets across the Mediterranean to the middle of France, but never so far as England.

Mr. Williams. May I add one remark? There were two distinct sunset-glows almost every evening. First of all we had the ordinary sunset-glow, which we may call the normal sunset-glow, illuminating the clouds, and then in a few minutes, perhaps a quarter of an hour after sunset, it has passed away, and those clouds have become dark. Another and a brighter glow has appeared when the Sun has gone down perhaps half an hour, and it illuminates the atmosphere at a height far above those clouds, and those clouds which in the first glow were of a rosy or red colour have become black and projected on the second glow. I noticed that particularly in the early part of this week. I watched it from the Harrow road, near the London and North-western Railway, and as the railway engines puffed out their steam, it was projected black or purple on the later glow and was perfectly opaque. The idea presents itself that there may be an ordinary sunset-glow due to what may be in our atmosphere, perhaps in some of our ordinary particles of dust, and another one beyond our atmosphere; but it appears to me that this meteoric cloud which we are going through, if it be one, must be of vast extent, and it may be a part of our solar system prevailing between us and the Sun. But the question of course would lead to any amount of speculation; but the question about dust from Java is to my mind hardly discussable; because if something had been thrown up into the atmosphere, unless it were charged with hydrogen gas it must come down long before three months had elapsed, and there would, at any rate, be a gradual diminution; but whatever it is, it is still going on pretty nearly the same, and there has not been much diminution. The last week or two have given us some splendid displays. The conditions are that there should be no clouds in the neighbourhood of the Sun. To-night, coming to this meeting, I observed a red glow round the Moon at a distance of about 10 degrees. I saw it very distinctly when the Moon was below the house-tops; it was not a halo, but a rosy tint very different from it.

Mr. Knobel. The only observation I have made with care was on Tuesday last, when there was a brilliant display of red. I had a very clear horizon to the south-west, and no house within 15 miles. I was able to determine and watch the disappearance of the redness. I was able to watch it right down to the horizon, and to take the time of its disappearance with no greater error than 2 minutes, The redness disappeared when the Sun was not more than $10° 20'$ below the horizon.

Mr. Finch suggested that the velocity of propagation of the particles might be quite sufficient to enable them to travel in the direction of the Trade-winds so far as the West Indies by September 2. But if it was objected that the particles ought to have travelled as far north or south in the same time, it might be answered that the action of the Trade-winds coming from north and south would have retarded motion in those directions. With regard to the particles, if they were supposed to be little globules filled with gas, it should be remembered that being intensely hot at the time of their formation they would be extremely light, and would therefore be a long time in coming down. With regard to the corresponding phenomenon of the lighter particles carried down by rivers, Dr. Routh had shown the immense time taken for the fine particles in rivers to be deposited in the sea, so that it might extend into years. There need be no difficulty, therefore, if these globules were filled with intensely rarified air, in their remaining a long time in the upper regions of the atmosphere.

Mr. Ranyard. The difficulty is that they are not pumice, but they are iron; and not small particles, but large ones. I do not know that they may not have come from a volcanic region. There is a quantity of silex in the dust brought down by the snow, a quantity of black oxide, and some iron particles, but no carbonate of lime. In some volcanic dust there is carbonate of lime; but in these specimens there is none.

Mr. Bryant. Have these phenomena been noticed at sunrise?

Mr. Ranyard. Oh! repeatedly.

The President. Amongst all these speculations we should remember that there has been a considerable disturbance in our atmosphere. All the stations round the world have registered storms, and there seems to be a serious barometrical disturbance. At all events the velocity of propagation can be determined with considerable accuracy, and these barometrical disturbances are certainly connected with the Java outbreak; but whether the sunset phenomena are due to this dust brought to us from Java or not, I am not prepared to offer an opinion.

The following papers were also announced:—

A. Marth. "Ephemeris for finding the Positions of the Satellites of Uranus, 1884."

W. F. Denning. "The Rotation-period of Jupiter."

Gen. G. H. Willis. "Note accompanying a drawing of the Great Comet *b* 1882 (an extract from a letter to Mr. Stone).

Dr. J. Morrison. The Orbit of the Great Comet *b* 1882.

The following gentlemen were duly elected Fellows of the Society:—C. H. Clarke, B.A., Joseph Morrison, M.A., M.D., and Major C. H. Fisher.

The Meeting adjourned at $10^h\ 0^m$.

Meeting of the Liverpool Astronomical Society.

The third Meeting of the Session was held on Monday, 17 December. The Rev. T. E. Espin, B.A. (Vice-President), in the Chair. Ten Members were elected and fourteen Candidates proposed. The Chairman said that a parcel of books had been presented to the Society by Dr. Ball, Astronomer Royal for Ireland. This gift, which was in itself valuable, was rendered still more so by the kindly expressed letter accompanying it. He had great pleasure in moving that a special vote of thanks be accorded to Dr. Ball. (Applause.) This was seconded by Rev. J. H. Honeyburne, M.A., and carried unanimously. A paper on the proper motion of Sun-spots was contributed by Miss E. Brown. The observations were made, on 8 Nov. last, by projecting the Sun's disk on a sheet of white paper. Two small spots were first visible near the E. limb, followed by a larger one, then just coming into view. By the time these spots had advanced fairly on the disk they formed a well-marked group, and it was plainly evident that the two first observed were much nearer to each other than to the third. But on the 12 Nov. the relative position of the three spots was altered, the central one having gone backwards till it was about halfway between the other two. This seemed clearly to show a retrograde motion in the central spot; and it may be as well to remark that, during this time, neither of the spots exhibited any changes of outline, as is so frequently the case. The positions and shape of the spots were illustrated by carefully executed diagrams.

A paper by Mr. J. E. Gore, F.R.A.S., was read. The remarkable variable star χ Cygni had lately passed through a maximum. The period of its variation was not uniform, but, on the whole, the observations showed that it had been slowly increasing. At its brightest it seemed to vary from the 4th to the 6th mag., but is usually about 5 mag., as at the recent maximum. The variable is Bayer's χ Cygni, and has been frequently confused with 17 (Fl.) Cygni, to which Flamsteed affixed the letter χ by mistake, the variable, which is the true χ, having been faint at the time. Referring to the previous paper, Mr. Espin said, a request was received from Mr. Gore in September to observe the maximum. The systematic observation had been taken by Mr. Gage. The comparison stars were 17 Cygni, of 5·2 mag., and 8 Cygni of 4·6 mag.; but most of the estimations were made from 17 Cygni. The value of different stations, widely apart, was shown from the fact that, while during a fortnight the weather was unfavourable at Ballisodare, observations were secured on four nights at West Kirby. There were some discrepancies in the resulting magnitudes, but these were probably due to the plan they adopted of placing the opera-glass out of focus, which, in a great degree, eliminated the colour. The variable is a fine orange, and he had found the difference between the brightness in and out of focus was about half a magnitude for orange stars. A paper was read by the Rev. S. J. Johnson, F.R.A.S., on the After-glow and Auroræ. The extraordinary character of the dawn and sunset for some ten days previous to the 6 Dec. had attracted much attention. The rule had lately been, that, for an hour after sunset and an hour before sunrise, the heavens had been suffused with an intensely ruddy vapour, the illumination being often grand, yet no actual aurora had been witnessed, at any rate in Dorset. The nearest approach to an aurora was on Saturday, 1 Dec., when at $5^h\ 15^m$ a very fine semicircular auroral cloud stretched across the sky from a point due N. to the S.W., ending in a mass of cloud at a low altitude. After a few minutes the cloud passed toward the E. and died away. In the W. there was at the time a ruddy cloud indented with dark finger-shaped rifts; the whole thing had probably been an edition of what is termed the "after-glow," referred to by Sir J. Herschel. That it had not been auroral was clearly shown by the absence of flickering or streamers.

Mr. W. E. Jackson wrote that in Constantinople they had had the most glorious sunsets imaginable. On the 1st Dec. the crescent moon, then a little more than 1·8 days old, was, in his 4-in. refractor, of a pale green, and the bright limb seemed to extend to an extraordinary distance round the dark body. On the 4th, the Moon, Fomalhaut, and Vega seemed to float in a rose-coloured sea, and the thin fleecy clouds, which slowly drifted across the Moon's face, were of a beautiful pale green.

Mr. Gemmill, in a short paper on 95 Herculis, directed attention to the estimates of the colours of its components. He said that, as these colours had been put upon record by different observers, they seemed to differ greatly, and, as a consequence, it has been suspected that the colour of one or both is variable. He thought, however, that many of the apparent discrepancies were due to the want of a reliable standard of colour.

In a communication to the Society, Mr. A. Stanley Williams called attention to the effect of mist in altering the relative brightness of lunar objects. He had lately been giving some attention to the determination of the brightness of different objects on the Moon, basing his estimation on the standard objects photometrically measured by Prof. Pickering at Harvard. To take what was perhaps an extreme instance, a slight mist or haze would spread over the sky, and the standard objects, Pitatus and Mercator, of 4·6 and 4·4 mags. respectively, would be scarcely distinguishable in brightness from Billy and Grimaldi, of 5·4 and 5·2 mags. Again on a night to all appearance precisely similar, whilst the two first-named objects would be of their normal brightness, the two latter would appear almost perfectly black and much darker than they should. Even in the pure sky of Brighton these curious changes were a great inconvenience and source of possible error; but on coming to London, the whole phenomenon was greatly intensified, and the standard objects were in a state of almost constant change, sometimes altering from one extreme to the other in the course of a few minutes.

The Period of the Double-Star Delta Equulei (OΣ 535)*.

The $4\frac{1}{2}$ magnitude star δ Equulei has been known as a wide pair since the observations of Sir William Herschel, by whom it was first measured in 1781. It was subsequently measured by South, and later by W. Struve and others. As a double-star in these respective catalogues it is known as H. IV. 37 = S 782 = Σ 2777.

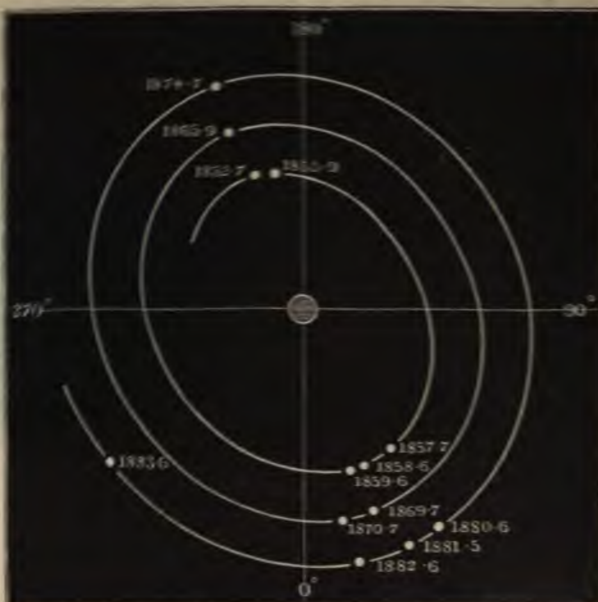
In 1852 Otto Struve discovered that the principal star of the wide pair was itself an excessively close pair, and it was measured by this observer at the Pulkowa Observatory a number of nights between that time and 1874. It was evident that the motion was rapid, and the period probably as short as that of any pair known, but the measures were isolated and scattering, and too uncertain as to the quadrant to give any definite result, although it was supposed the period was some multiple of six or seven years. In 1880 I made a set of measures on five nights, under very favourable conditions, with the $18\frac{1}{2}$-inch Refractor of the Dearborn Observatory. The components were then well separated by that refractor, the distance being a little less than 0″·4, which is about their maximum distance. These measures have been repeated each year since down to the present time. The distance now is only about 0″·2, and it will therefore appear single in all but the largest and best refractors. These later measures are important in this investigation, since they not only give definitely the direction and the rate of motion in this part of the orbit, but also fix with certainty the direction of the smaller star, which was very doubtful, to say the least, from the prior observations. The components are so nearly equal that the difference, which is only about three-tenths of a magnitude, would not be apparent in so close a pair with the ordinary telescope used. With the single exception of my observations of the present year, all the measures have been made in about the same angle (± 180°), or at the maximum distance of the stars. By making use of the later measures and taking them as the starting point, it seemed possible to work back and so correct the earlier ones for quadrant and arrange the whole as to give a closely approximate period. For this purpose it is not necessary to consider the distances, which in many cases are only estimated, or roughly measured. All the published measures to this date are given below. They have been made at Pulkowa and Chicago, with the exception of those of 1869 by Duner and 1870 at the Harvard College Observatory. A correction of 180° has been applied to the angles marked (*).

	°		
1852·66	200·6*	OΣ	2 nights.
1853·91	191·9	OΣ	1 night.
1857·67	29·5*	OΣ	2 nights.
1858·59	16·8	OΣ	1 night.

* From the 'Sidereal Messenger,' 1883, November.

	°		
1859·65	15·5	OΣ	1 night.
1865·91	205·3	OΣ	1 night.
1869·74	15·9*	Harv. Coll.	6 nights.
1870·73	8·0	Duner	1 night.
1874·72	202·3*	OΣ	3 nights.
1880·60	29·1	β	5 nights.
1881·46	22·1	β	5 nights.
1882·63	9·8	β	3 nights.
1883·55	307·6	β	3 nights.

These measures are shown in the following diagram:—



If the measures are reasonably accurate, and the proper quadrant has been assigned to those made previous to 1881, it is apparent that the period must be much shorter than that of any other pair now known. They would seem to indicate, when taken together, a period of about 10·8 years. Measures made in 1885, when the smaller component should be at nearly maximum distance in the opposite quadrant, will show positively whether any of the angles have been erroneously reversed in this diagram. I think there can be no error in this respect; but it is desirable that careful measures with adequate instruments be made each year when the distance is sufficient for the purpose. At the greatest elongation there should be no difficulty in observing it with an ordinary instrument of the best definition. It will certainly be very close and perhaps un-

On the possible Connection of the Pons-Brooks Comet with a Meteor-stream.*

I DESIRE to call attention to some slight evidence of the existence of a meteor-stream which may possibly stand in some sort of connection with the Pons-Brooks comet. From an examination of all the available material of published meteor-tracks in the interval Dec. 5–8, I find that, after excluding those manifestly emanating from the well-known and active radiants in Andromeda, Gemini, and Taurus, there remain twenty-three meteors observed by Heis on Dec. 8 (about two-thirds of them in 1847 and the rest in 1855, 1857, and 1867), and ten meteors observed at Vienna, Dec. 7, 1868; all of which indicate a strongly marked radiant in Draco. From these data I have carefully determined the position of this radiant as follows:—

	R.A.	Decl.	Long.	Lat.
10 meteors on Dec. 7 ..	198°·0	+72°·0	135°·0	+65°·6
23 meteors on Dec. 8 ..	190·0	+69·7	137·2	+62·4

and from these I derive the following orbits, to which I add for comparison that of the Pons-Brooks comet:—

	Meteors of		Pons-Brooks Comet, 1884.
	Dec. 7.	Dec. 8.	
T=Perihelion passage..	Dec. 28.	Dec. 23.	Jan. 25·82.
Long. of perihelion	44°·5	55°·1	93° 21′
Long. of node	256 ·1	256 ·3	254 6
Inclination	68 ·5	72 ·7	74 3
Log. per. dist.	9·9600	9·9784	9·8894
Eccentricity	....	....	0·9550

The resemblance is thus not sufficient to give any considerable probability to the hypothesis of an intimate relation. On the other hand, the position of the radiant from present data is too uncertain to enable us to pronounce against that hypothesis.

The differences in inclination and longitude of perihelion are not greater than are due to uncertainty in the observed radiant-points: the T and the node are, of course, of no significance in the comparison. The descending node of the comet's orbit lies at the distance 0·200 inside the Earth's path, and the difference of the perihelion-distance of the comet and the meteors is about 0·15. There is

* From 'Science,' Vol. ii. No. 26.

nothing in our present knowledge of the dimensions of meteor-streams, or of the nature of their relations to comets, definite enough to render such a breadth as is here implied evidence against a possible connection. On the whole, therefore, it appears desirable that meteor-observers should give close attention to the radiant in question about the date of the Earth's passage through the plane of the comet's orbit, Dec. 5 to Dec. 7. Observations this year are likely to prove specially instructive, on account of the proximity of the comet, which passes the node only a few weeks later.

Harvard College Observatory, 1883, Nov. 12. S. C. CHANDLER, Jun.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The so-called New Star of A.D. 389.

SIR,—

In the 10th edition of the 'Outlines,' p. 602, we read :—

"Such was the star which appeared A.D. 389 near α Aquilæ, remaining for three weeks as bright as Venus, and disappearing entirely."

It is somewhat singular that it did not occur to Sir John that the above statement is too precise to be accurate, as the names Altair and α Aquilæ were not given to that star until centuries after the date in question, and the chroniclers of that era were not likely to indicate the position of a star very closely. Arago, in his 'Astronomie Populaire,' contents himself with saying that it was near the constellation Aquila ("vers l'Aigle"). There seems, however, to be little doubt that the so-called star was in fact a comet ; if a veritable star, it certainly was not in or near Aquila.

In the 'Chronicon' of Count Marcellinus we read, under date A.D. 389, "Theodosius imp. cum Honorio filio suo Romam mense Junio introivit' [after his victory over Maximus], "congiarium Romano populo tribuit, Urbeque egressus est kal. Septembris. Per idem tempus grando crepitans per biduum continuum pro pluvia cecidit, pecorum arborumque pernicies. Stella a septentrione gallicinio surgens, et in modum Luciferi ardens potius quam splendens apparuit, vicesimo tertio die [*al.* vicesimo die] esse desiit." Not to mention the (roughly recorded) position of the object in question, it is evident that no star near the constellation Aquila could have risen at cock-crowing or in the early morning in any part of August or September.

The so-called "star" thus referred to was doubtless the same object as that described in the Ecclesiastical Histories of Philostorgius and Nicephorus Callistus, which was unquestionably a comet, and is referred to as such by Mr. Chambers in his 'Descriptive Astronomy.' It appeared, Philostorgius tells us *, near the morning

* Epitome of Photius, lib x. ch. 9.

star (Eōsphorus), to which it was not much inferior in brightness, and was first seen in the middle of the night in the circle called the zodiac. (By the expression κατὰ μέσας νύκτας we need probably only understand some considerable time before daylight.) Afterwards, he says, there took place a rush (συνδρομή) of the stars in its neighbourhood towards it, like a swarm of bees clustering around their "king"; in other words, it underwent a considerable increase of brightness, which was succeeded by other changes in appearance similar to those with which we are familiar, as distorted in early descriptions of comets. Finally it moved towards the north, and disappeared in the midst of Ursa Major.

I need not refer to the account of Nicephorus, it being derived from that of Philostorgius, whose history was published about thirty-six years after the appearance of the comet, which he had probably seen himself. The latter is in fact the only contemporary authority extant, as Marcellinus wrote about 150 years afterwards, in the reign of Justinian.

Doubts have been raised whether the star mentioned by the poet Claudian in the well-known lines:—

"Visa etiam medio, populis mirantibus, audax
Stella die, dubitanda nihil, nec crine retuso
Languida, sed quantum numeratur nocte Bootes,"

was the same as this. It is very difficult to make out from Claudian at what date this wonderful star appeared. The lines occur in his poem written in honour of the fourth consulship of the Emperor Honorius, which was in A.D. 398 (Theodosius died in January, 395), but it by no means follows that he is speaking of what occurred in that year. Licetus thinks that they were different stars; but his opinion is not entitled to much weight. Tycho Brahe considered that the star mentioned by Claudian was in fact the planet Venus, seen in the daytime. And it appears to me, though Licetus argues against it, that this is not unlikely. At any rate the description of Claudian is too vague to draw any definite conclusion from; whilst, as I said before, it is impossible to say at what date the "stella audax" appeared, and it may have been the comet of A.D. 389, described by Philostorgius. But we are nowhere told that that comet was visible in the daytime; and I incline to Tycho's view that Claudian's star was the planet Venus, seen in the morning after sunrise. "Medio die" is doubtless a loose description of the time of day.

Yours faithfully,

Blackheath, 1883, Nov. 19. W. T. LYNN.

P.S.—I have traced the mistake about the "star" of A.D. 389 to the 'Kosmos' (vol. iii.), and probably Humboldt originated it. He refers for anthority to Crespinianus, who (he says) saw the "star" himself. Now Crespinianus (whose real name was Spieshammer) was not born until 1473, nearly eleven centuries after the appearance of the object in question.

The asserted Volcanic Haze.

SIR,—

On the chance of your caring to find place for it, I may as well send you a remark upon one effect of the red sunset haze which was latterly attracting so much attention. It is that, whereas an auroral haze of similar general appearance will often improve the definition of celestial objects, the haze referred to has shown a decidedly opposite tendency. I especially watched α Herculis, as a suitable test-object low in the western sky, and repeatedly found the image so blurred that duplicity was scarcely recognizable, at altitudes at which any ordinary haze would have transmitted a tolerable result. The instrument used was an equatoreal with a fine $6\frac{1}{8}$ Calver mirror, which easily divides the outer ring of Saturn, and which, a few nights ago, through (I had almost said owing to) a mist sufficiently dense to veil the Hyades from the naked eye, showed me Enceladus.

The spectroscope appeared to be altogether baffled, but possibly might have yielded a better result to one less inexperienced in its manipulation.

While on this subject, I would venture to add that where an observer is confident he is looking at a green moon in a crimson sky, a glance through an empty tube, such as a roll of paper, will commonly dispel the illusion.

East Clevedon, Somerset,
1883, Dec. 10.

Yours faithfully,
STEPHEN H. SAXBY.

The Central Star in the Trapezium of Orion.

SIR,—

My attention has been called to an article in the 'English Mechanic,' December 7th, by Mr. James Dunlop, on his review of Dr. Klein's work, in which the writer does me the honour to quote my letter which appeared in the 'Observatory' some months since, as it regards the star in question. Mr. Dunlop intimates some doubts as to the reality, as it has not been observed by astronomers with larger instruments than mine.

But allow me to say that on the night I saw it, the atmosphere was exceptionally free from tremulous motion, so that the appearance of a star could not have been caused by "coruscations &c." from other stars!

I would further add, the *coloured* star in the southern boundary of the Trapezium (direct vision) was very conspicuous.

Ipswich,
1883, Dec. 17.

Yours truly,
JEREMIAH BYLES.

Mean Longitude of the Sun.

SIR,—

The difference between my views and those of Professor Adams appears to be briefly as follows:—Professor Adams assumes that we use in our practical astronomy the true mean solar day as our unit, although, as a matter of fact, we do change the adopted value of the Sun's mean motion in longitude. I say that this is impossible. We construct our Solar Tables on the assumption that in each day, adopted day, the Sun's mean longitude increases some *assigned* and definite angle, n_1. When, therefore, we change this assumption, and assert that in each day, adopted day, the Sun's mean motion increases n_2, the two assumptions are perfectly inconsistent with each other if we are speaking of any real motion, and not of the motions of imaginary or arbitrary Suns, unless we admit that the adopted days referred to are not the same intervals of time, but two different intervals, T_1 and T_2, such that $n_1 : n_2 = T_1 : T_2$.

Now Professor Adams will not recognize the difference between T_1 and T_2, and he is thus driven either:

(1) To assume that there are two Suns, with Professor Cayley;

(2) That he has nothing to do with the direction in space of his meridian at the instant of mean noon, nor with the longitude of the Sun at mean noon, nor with the longitude of the Moon at mean noon, nor, it appears to me, with practical astronomy.

As a matter of fact we do determine our mean times through our sidereal times, and our mean sidereal times depend directly upon the position of the meridian with respect to the mean equinox.

Radcliffe Observatory, Oxford, 1883, Dec. 24.

Yours truly,
E. J. Stone.

Atmospheric Effect preceding Sunrise.

Sir,—

Although the atmospheric effects exhibited after sunset on the evenings which closed the month just passed were of a very extraordinary character, I venture to think the effect which preceded sunrise this morning still more phenomenal and exceptional. From 7.15 to 7.30 the western portion of the country was bathed in a strong light of extraordinary clearness: the transparency of this was most marked—the hills, houses, and trees stood out sharply and without any softened outline; the sky overhead was clear and of a peculiar greenish hue. The light more resembled an electric one than any other I can compare it to. As the day broke, these appearances gradually melted away. At 8.11 the first glimpse of the Sun was caught as he rose above the hills. We have, then, this curious condition of things—a brilliantly defined light 55 minutes before actual sunrise. This is most exceptional, and can only arise from reflection taking place from some medium at an elevation far above the earth's surface. It is difficult to conceive what this medium can be, for this morning was not only conspicuous for its clearness, but for its absence from humidity, and consequently from the presence of those aqueous-bearing particles which so strongly help to reflect and disperse the light-rays; the temperature was 36° F., and a fairly strong wind blowing from N.W. There are no signs of solar activity, the Sun having on its surface two spots only—one small one on the margin, and another near the centre exceedingly minute.

Audley, Bath, Dec. 4th.

J. L. Stothert.

NOTES.

Observations of Jupiter at the Dearborn Observatory, Chicago.—The 'Report of the Chicago Observatory' states that the planet Jupiter has been carefully watched as usual, the principal object of interest being the great red spot first noticed in 1878. This object "has been remarkable in its stability. Since the first observation made here in September 1879 to the present time, it has not changed very materially in length, breadth, outline, or latitude. There has been, however, a slow retrograde drift in longitude, or, in other words, a gradual increase in the apparent time of rotation of the planet on its axis. The rotation-period in 1879 was $9^h\ 55^m\ 34^s{\cdot}0$, but it has increased continually, and the value from the past opposition is $9^h\ 55^m\ 38^s{\cdot}4$." Prof. Hough gives the following mean results for the dimensions and latitude of the spot:—

	1879.	1880.	1881.	1882.
Length ..	12″·25	11″·55	11″·30	11″·83
Breadth ..	3 ·46	3 ·54	3 ·66	3 ·65
Latitude ..	−6 ·95	−7 ·14	−7 ·40	−7 ·52

The spot, therefore, has gradually drifted south 0″·5, or about 1200 miles. The south edge of the great equatoreal belt has, however, moved much more rapidly north during the past opposition, and it is now nearly coincident with the middle of the spot, yet the two objects have always remained quite distinct and separate, a depression being formed in the edge of the belt, corresponding in shape to the outline of the spot, the distance between the two being about one second of arc. Prof. Hough observed a satellite in transit cross the red spot on 1880, July 3, and during the last opposition he observed a satellite transit over the densest portion of the equatoreal belt. In the latter instance the satellite was lost as soon as it would have been on the bright part of the planet; in the former it seemed as bright as when off the disk. Prof. Hough refers to the great spot seen by Hooke and Cassini in 1664–5–6, which occupied nearly the same position in latitude as the red spot, and which appeared and vanished eight times between 1665 and 1708. "If the ancient observation, extending over half a century, refers to the same object, we would naturally infer that it was a portion of the solid body of the planet; being sometimes rendered invisible by a covering of clouds." The great equatoreal belt which remained without material change in size or position in 1879, 1880, and 1881, drifted south nearly 2″ of arc during the lost opposition; its northern edge was not well defined, so that it appeared to extend up North Latitude +8″. The observations of the equatoreal white spot first observed in 1879 were satisfied by the same rotation-period as deduced last year, viz. $9^h\ 50^m\ 9^s{\cdot}8$. "It is a curious fact that these equatoreal spots should drift for years with the enormous velocity of 260 miles per hour, if they are nothing more than clouds in the atmosphere of Jupiter." Prof.

Hough again puts forward his view that "statements of change in size or shape of objects on the disk of Jupiter in the course of a couple of hours are not legitimate deductions drawn from the actual phenomena," the apparent changes being due, according to him, to foreshortening, and to the rapid rotation of the planet on its axis.

The Great Comet of 1882 and difficult double stars were the principal objects of observation beside Jupiter. On Oct. 6 there were several centres of condensation visible in the Comet's head, besides the principal one; but each and all of these masses were connected by matter of less density, there was no actual separation. Similar observations were made as late as 1883, March 6, and Prof. Hough states that all the observations made at Chicago confirm him in the belief that no complete separation took place between the parts of the head.

The companion of Sirius was measured on eleven nights. Distance $9''\cdot04$, position-angle $39^\circ\cdot9$, for epoch 1883·12. The distance between the components diminishes about $0''\cdot3$ annually, so that in a few years it will be entirely out of the reach of any except the largest telescopes.

The Telluric Lines of the Solar Spectrum *.—M. Egoroff, in continuation of his studies on the Telluric Spectrum, has been able to show that the A and B groups in the solar spectrum are due to the oxygen in our atmosphere. The spectrum of a Drummond light viewed through a tube, 20 metres in length, filled with air at pressure of 5 atmospheres, showed A distinctly; but under a pressure of 8 atmospheres, the band became broader and more distinct. As oxygen was added to the air in the tube, the group became darker and more defined, and pure oxygen showed the group very well even at a pressure of 1 atmosphere, but at 3 atmospheres it was seen as distinctly double. At 6 atmospheres group B was seen, and with further increase of pressure both groups increased in intensity and extent. Hydrogen at 3 atmospheres showed no line at all in the visible part of the spectrum. M. Egoroff was not able in the least to confirm Capt. Abney's view that A and B were due to hydrocarbons in interplanetary space, his experiments failing to show any trace of absorption lines or bands when his absorption-tube was filled with coal-gas or air saturated with benzine.

Close of 'The Analyst.'—With the number for 1883 November, the 'Analyst,' the American journal of pure and applied mathematics, edited with so much ability for the last ten years by J. E. Hendricks, A.M., came to a close, Mr. Hendrick's health not permitting him to continue its publication. Profs. Ormond Stone and W. M. Thornton, of the University of Virginia, have,

* Comptes Rendus, Vol. xcvii. No. 9.

however, announced their intention of shortly publishing a new magazine as a continuation of the 'Analyst,' under the title of 'Annals of Mathematics, Pure and Applied.' The first number will be issued Feb. 1, 1884, and it will appear at intervals of two months. The office of publication will be at the University of Virginia.

Mountain Observatories: The Pic du Midi *.—During the past summer MM. Thollon and Trépied spent five weeks at the meteorological observatory on the summit of the Pic du Midi, in order to test the suitability of the station as a site for a permanent astronomical observatory, and the observations they there made convinced them that science would gain much by the completion of such an observatory there. On the mornings of Sept. 19 and 20 they were able to distinguish the planet Venus with the naked eye when but 2° from the Sun. The definition obtained in the mornings before the slopes of the mountains had become heated by the Sun was marvellous; "the limb of the Sun projected on the slit of a spectroscope showed a spectrum with a boundary as sharp as if produced by a punching-machine;" and at night the perfect definition observed in the case of the Sun was reproduced with the moon, planets, and stars.

The spectroscopic observations were especially interesting, lines corresponding to the granulations of the solar surface and running the length of the spectrum being seen under favourable circumstances †; and it was remarked that the hydrogen lines C and F seemed to be composed of distinct dark and bright fragments corresponding in size to the intervals between the transversal lines. This phenomenon was observed constantly over the entire disk; and prominences also were frequently observed on the full disk, it being possible to open the slit sufficiently widely to observe their shapes. The chromosphere too, instead of showing only eight bright lines altogether, revealed more than thirty between D and F. Their wave-lengths were as follows:—

5533·6	5273·2	5204·8	5122·6
5525·8	5258·9	5199·5	5114·4
5469·9	5254·3	5196·9	5112·1
5361·5	5252·2	5183·0	5087·0
5324·3	5248·8	5172·0	5029·8
5318·7	5233·9	5168·3	5017·9
5292·4	5225·6	5166·7	4983·6
5283·1	5207·4	5147·0	4923·0
5275·0	5206·8	5130·2	4882·9
			4854·2

* Comptes Rendus, Vol. xcvii. No. 16.

† [These lines may easily be seen on any fine day with a good "half-prism" spectroscope reversed so as to give purity; but the detection of changes so minute in C and F certainly implies a marvellous sharpness of definition.—Editor.]

Approximate Times of Transit of the great Red Spot and Equatoreal White Spot across the Central Meridian of Jupiter in January 1884.

Day of Month.	Red Spot on C.M.	White Spot on C.M.	Day of Month.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	9·7	14·7	17.	12·9	14·3
2.	15·6	10·4	18.	8·8	9·9
3.	11·4	15·9	19.	14·6	5·6
4.	17·2	11·6	20.	10·4	11·1
5.	13·0	7·3	21.	6·3	6·8
6.	8·9	12·7	22.	12·1	12·3
7.	14·7	8·4	23.	7·9	8·0
8.	10·5	13·9	24.	13·7	13·4
9.	16·3	9·6	25.	9·5	9·0
10.	12·2	15·1	26.	15·3	14·6
11.	8·0	10·7	27.	11·1	10·3
12.	13·8	6·4	28.	7·0	6·0
13.	9·6	11·9	29.	12·8	11·5
14.	15·4	7·6	30.	8·6	7·2
15.	11·3	13·1	31.	14·4	12·6
16.	7·1	8·7	Feb. 1.	10·3	8·3

I observed the red spot crossing the planet's central meridian at the following times:—

1883.	On C.M.	Long. of Spot.
November 29............	$17^h\ 38^m$	$75^\circ{\cdot}0$
December 4............	16 48	77 ·4
" 5............	12 41	78 ·6
" 19............	14 9	79 ·2

It now follows Mr. Marth's first meridian ('Monthly Notices,' June 1883) by about $2^h\ 11^m$, and the interval continues to increase.

The equatoreal white spot came to the C.M. on December 19 at $12^h\ 3^m$, when it was very brilliant. The recent observations of this marking compared with those of 1880 show that the period is increasing, and that therefore the spot is drifting in E. longitude. The rate of this increase seems to be even greater than that found in regard to the red spot.

On Dec. 23, 1882, the longitude of the white spot, computed on the period of $9^h\ 50^m\ 7^s{\cdot}42$ ($=878^\circ{\cdot}46$ daily rate), which Mr. Marth found to satisfy the observations between Oct. 1880 and July 1882, was $32^\circ{\cdot}9$; but on Dec. 19, 1883, it had increased to $75^\circ{\cdot}2$. Relatively to the above period of rotation the spot has therefore lost $1^h\ 10^m$ during the 12 months, which is equivalent to an increase of nearly 5 sec. in the rotation-period. This slackening motion of the spot has caused it to arrive at the C.M. somewhat later than the times in our monthly lists.

As to the red spot it continues to be fairly well defined. It will be in conjunction with the white spot on or about January 24.

Bristol, 1883, Dec. 20. W. F. Denning.

Ephemeris of Comet Pons-Brooks.*
By MM. Schulhof and Bossert.

For Berlin Midnight.

	R.A.	Dec.	Log Δ.	Brightness.
1883.	h m s	° ′ ″		
Dec. 30....	21 31 41·65	+25 29 14·0	9·83193	83·93
1884.				
Jan. 1....	21 46 6·64	22 27 6·3	9·82165	
3....	22 0 24·13	19 10 57·6	9·81334	99·31
5....	22 14 28·03	15 42 37·4	9·80673	
7....	22 28 12·54	12 4 37·6	9·80376	112·00
9....	22 41 32·58	8 20 4·8	9·80285	
11....	22 54 23·80	4 32 26·2	9·80462	119·22
13....	23 6 42·62	+ 0 45 12·7	9·80897	
15....	23 18 26·50	− 2 58 17·7	9·81573	119·54
17....	23 29 33·75	6 35 14·4	9·82462	
19....	23 40 3·48	10 3 22·7	9·83530	113·51
21....	23 49 55·55	13 21 7·5	9·84743	
23....	23 59 10·46	16 27 31·1	9·86065	103·11
25....	0 7 49·30	19 22 8·2	9·87463	
27....	0 15 53·54	22 4 59·4	9·88906	90·61
29....	0 23 25·06	24 36 24·3	9·90370	
31....	0 30 25·97	−26 56 55·4	9·91833	77·80

The brightness of the comet at the time of discovery is taken as unity.

Differences of Right Ascension and Declination between Iapetus and the centre of Saturn. By A. Marth†.

	α−A.	δ−D.		α−A.	δ−D.
	s	″		s	″
Jan. 1	+32·2	+218	Jan. 17	−14·3	+108
2	+30·0	+221	18	−17·1	+ 93
3	+27·6	+222	19	−19·9	+ 77
4	+25·1	+222	20	−22·5	+ 61
5	+22·4	+220	21	−24·9	+ 44
6	+19·6	+217	22	−27·2	+ 27
7	+16·7	+213	23	−29·3	+ 10
8	+13·7	+208	24	−31·2	− 7
9	+10·7	+201	25	−32·8	− 24
10	+ 7·5	+193	26	−34·3	− 41
11	+ 4·3	+184	27	−35·5	− 57
12	+ 1·2	+174	28	−36·4	− 73
13	− 2·0	+162	29	−37·1	− 89
14	− 5·2	+150	30	−37·6	−103
15	− 8·3	+137	31	−37·8	−117
16	−11·3	+123	Feb. 1	−37·7	−131

* Astron. Nachr. Nos. 2558 and 2560.
† Monthly Notices, Vol. xliii. No. 8.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

Mimas (East).

		h
Jan.	1	6·6
	14	11·2
	15	9·8
	16	8·4
	17	7·0
	28	14·5
	29	13·1
	30	11·7
	31	10·3

Mimas (West).

		h
Jan.	3	15·2
	4	13·8
	5	12·4
	6	11·0
	7	9·7
	20	14·3
	21	12·9
	22	11·5
	23	10·2
	24	8·8
	25	7·4
	26	6·0

Enceladus.

		h
Jan.	1	8·3
	2	17·2
	4	2·1
	5	11·0
	6	19·9
	8	4·8
	9	13·7
	10	22·5
	12	7·4
	13	16·3
	15	1·2
	16	10·1
	17	19·0
	19	3·9
	20	12·7
	21	21·6
	23	6·5
	24	15·4
	26	0·3
	27	9·2
	28	18·0
	30	2·9
	31	11·8

Tethys.

		h
Jan.	2	3·0
	4	0·3
	5	21·6
	7	18·9
	9	16·2
	11	13·5
	13	10·8
	15	8·1
	17	5·4
	19	2·7
	21	0·0
	22	21·3
	24	18·6
	26	15·9
	28	13·2
	30	10·6

Titan.

		h
Jan.	2	16·0
	18	13·4

Hyperion.

		h
Jan.	13	22·8

Dione.

		h
Jan.	3	4·8
	5	22·5
	8	16·2
	11	9·8
	14	3·5
	16	21·2
	19	14·8
	22	8·5
	25	2·2
	27	19·9
	30	13·5

Rhea.

		h
Jan.	3	14·0
	8	2·4
	12	14·8
	17	3·1
	21	15·5
	26	3·9
	30	16·3

The following table will furnish the means for inferring approximate distances and position-angles of the satellites from the diagram given in Plate I.

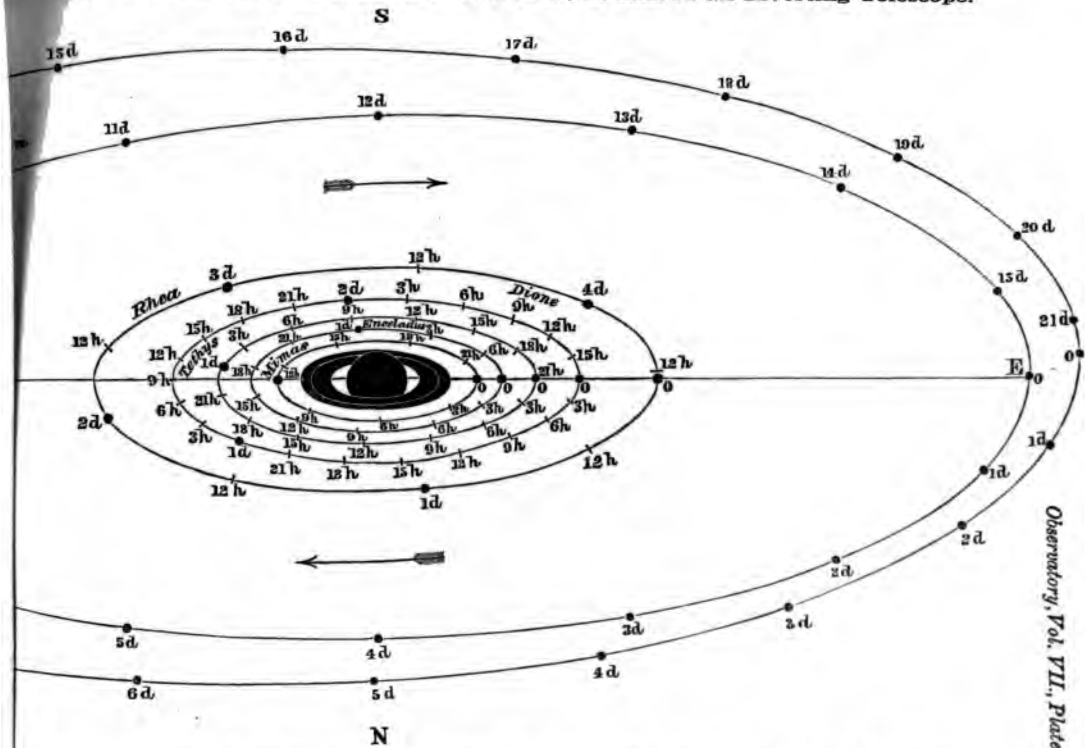
	Position-angle of Minor Axis of Ring.		Semi-major axis of orbit of Titan.
1883, Dec. 30	356° 35′	1883, Dec. 30	204″·8
1884, Jan. 9	356 39	1884, Jan. 9	202 ·2
19	356 42	19	199 ·2
29	356 43	29	195 ·8

Astronomical Memoranda, 1884, *January.*

Sun. Jan. 1, sets $4^h\ 0^m$, rises $20^h\ 8^m$; Jan. 11, sets $4^h\ 11^m$, rises $20^h\ 4^m$; Jan. 21, sets $4^h\ 27^m$, rises $19^h\ 55^m$; Jan. 31, sets $4^h\ 45^m$, rises $19^h\ 42^m$.

Equation of Time:—Sun *after* Clock, Jan. 1, $3^m\ 38^s$; Jan. 11, $8^m\ 2^s$; Jan. 21, $11^m\ 27^s$; Jan. 31, $13^m\ 38^s$.

SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



on of the motion of the Satellites. The figures indicate the interval from the time of last East elongation.

Sidereal Time at Mean Noon:—Jan. 1, $18^h\ 42^m{\cdot}2$; Jan. 11, $19^h\ 21^m{\cdot}7$; Jan. 21, $20^h\ 1^m{\cdot}1$; Jan. 31, $20^h\ 40^m{\cdot}5$.

Moon.	*sets.* h m		*sets.* h m		*rises.* h m
Jan. 1..	7 43	Jan. 11..	19 16	Jan. 21..	14 44
2..	8 55	12..	*rises.*	22..	15 43
3..	10 6	13..	5 56	23..	16 37
4..	11 22	14..	7 8	24..	17 27
5..	12 35	15..	8 20	25..	18 11
6..	13 52	16..	9 28	26..	18 51
7..	15 8	17..	10 34	27..	*sets.*
8..	16 21	18..	11 39	28..	5 29
9..	17 27	19..	12 42	29..	6 40
10..	18 27	20..	13 35	30..	7 55
				31..	9 9

First Quarter, Jan. 5, $9^h\ 35^m$; Full Moon, Jan. 12, $3^h\ 27^m$; Last Quarter, Jan. 19, $17^h\ 23^m$; New Moon, Jan. 27, $17^h\ 1^m$.

Mercury is an evening star in the first part of the month. He is at his greatest elongation, 19° 15′ E., Jan. 4, 11^h, and is in inferior conjunction with the Sun, Jan. 20, 8^h.

Venus is an evening star. Diameter:—Jan. 1, 11″·2; Jan. 31, 12″·3. Illuminated portion of disk, 0·883 on Jan. 15.

Jan. 1,	R.A. $20^h\ 34^m{\cdot}1$,	Dec. 20° 28′ S.,	tr. $1^h\ 52^m$,	sets	$6^h\ 4^m$
31,	22 58 ·8,	7 59 S.,	2 18		7 41

Mars is in opposition to the Sun, Jan. 31, 23^h. Diameter:—Jan. 1, 14″·8; Jan. 31, 16″·6. Illuminated portion of disk 0·986 on Jan. 15.

Jan. 1,	R.A. $9^h\ 40^m{\cdot}1$,	Dec. 17° 55′ N.,	tr. $14^h\ 55^m$,	rises	$7^h\ 15^m$
31,	9 5 ·3,	21 26 N.,	12 22		4 21

Jupiter is in opposition to the Sun, Jan. 19, 15^h. Diameter:—Jan. 1, 42″·6; Jan. 31, 42″·7.

Jan. 1,	R.A. $8^h\ 17^m{\cdot}2$,	Dec. 20° 14′ N.,	tr. $13^h\ 33^m$,	rises	$5^h\ 39^m$
31,	8 1 ·0,	21 8 N.,	11 18	sets	21 19

Saturn is retrograding in Taurus.

Jan. 1,	R.A. $4^h\ 10^m{\cdot}3$,	Dec. 19° 6′ N.,	tr. $9^h\ 27^m$,	sets	$17^h\ 15^m$
31,	4 6 ·2,	19 2 N.,	7 24		15 11

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Jan. 14	44″·62	19″·11	29″·67	12″·71	17″·8
Feb. 3	43 ·15	18 ·50	28 ·69	12 ·30	17 ·2

The south side of the rings is visible, the elevation of the Earth above their plane being 25° 21′ on Jan. 14, and 25° 23′ on Feb. 3; and of the Sun 26° 1′ and 26° 6′.

Neptune is stationary, Jan. 28, 21^h.

Jan. 1,	R.A. $3^h\ 6^m{\cdot}2$,	Dec. 15° 37′ N.,	tr. $8^h\ 23^m$,	sets	$15^h\ 48^m$
31,	3 5 ·3,	15 35 N.,	6 24		13 49

Phenomena.

		G. M. T. h	m	
Jan.	1	7	12	J. i. Tr. I.
		8	20	J. iii. Tr. I.
		9	32	J. i. Tr. E.
		11	55	J. iii. Tr. E.
	2	6	39	J. i. Oc. R.
		11	15	J. ii. Ec. D.
		14	59	J. ii. Oc. R.
	4	9	6	J. ii. Tr. E.
	5	16	53	J. i. Ec. D.
	6	8	15	54 Ceti Oc. D. 137°.
		14	30	J. i. Tr. I.
		16	50	J. i. Tr. E.
	7	11	21	J. i. Ec. D.
		13	57	J. i. Oc. R.
	8	8	56	J. i. Tr. I.
		10	50	B.A.C. 1206. Oc. D. 130°.
		11	16	J. i. Tr. I.
		11	37	J. iii. Tr. I.
		15	12	J. iii. Tr. E.
	9	8	23	J. i. Oc. R.
		13	52	J. ii. Oc. D.
		16	20	*m* Tauri Oc. D. 88°.
		17	14	J. ii. Oc. R.
	11	8	25	J. ii. Tr. I.
		11	19	J. ii. Tr. E.
	13	6	21	J. ii. Oc. R.
		7	20	A^2 Cancri Oc. D. 45°.
		11	55	60 Cancri Oc. D. 61°.
		16	14	J. i. Tr. I.
		18	34	J. i. Tr. E.
	14	9	22	J. iv. Oc. R.
		13	15	J. i. Ec. D.
		15	40	J. i. Oc. R.
	15	10	39	J. i. Tr. I.
		12	59	J. i. Tr. E.
		13	28	34 Sextantis Oc. D. 94°.
		14	53	J. iii. Tr. I.
Jan.	15	18	28	J. iii. Tr. E.
	16	7	44	J. i. Ec. D.
		10	6	J. i. Oc. R.
		16	28	J. ii. Ec. D.
	17	7	25	J. i. Tr. E.
	18	10	38	J. ii. Tr. I.
		13	33	J. ii. Tr. E.
		15	39	B.A.C. 4394 Oc. D. 68°.
	19	8	1	J. iii. Oc. R.
	20	5	41	J. ii. Oc. D.
		8	35	J. ii. Oc. R.
		17	37	5 Libræ Oc. D. 87°.
		17	57	J. i. Tr. I.
	21	15	4	J. i. Oc. D.
		17	24	J. i. Ec. R.
	22	11	8	J. iv. Tr. I.
		12	23	J. i. Tr. I.
		14	43	J. i. Tr. E.
		15	38	J. iv. Tr. E.
		18	8	J. iii. Tr. I.
	23	9	30	J. i. Oc. D.
		11	53	J. i. Ec. R.
		18	48	J. ii. Oc. D.
	24	6	49	J. i. Tr. I.
		9	9	J. i. Tr. E.
	25	6	22	J. i. Ec. R.
		12	52	J. ii. Tr. I.
		15	46	J. ii. Tr. E.
	26	7	41	J. iii. Oc. D.
		11	53	J. iii. Ec. R.
	27	7	55	J. ii. Oc. D.
		11	11	J. ii. Ec. R.
	28	16	48	J. i. Oc. D.
	29	14	7	J. i. Tr. I.
		16	27	J. i. Tr. E.
	30	11	14	J. i. Oc. D.
		13	47	J. i. Ec. R.
	31	7	49	21 Piscium Oc. D. 125°.
		8	33	J. i. Tr. I.
		10	53	J. i. Tr. E.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1884, January 11.

The Astronomer Royal, *Vice-President*, in the Chair.

Secretaries: J. W. L. Glaisher, M.A., F.R.S., and
E. B. Knobel.

The Minutes of the last Meeting were read and confirmed.

Mr. Knobel announced that 57 presents had been received since the last Meeting, but that none of them called for any special notice.

The thanks of the Meeting were unanimously voted to the respective donors.

Mr. Glaisher read a paper by *Mr. Tebbutt*, "On the latitude of the Observatory, Windsor, New South Wales." The former determination had given 33° 36′ 28″·9 S. as the latitude of the Observatory. Mr. Tebbutt's recent investigation gave 33° 36′ 29″·5, a difference of 0″·4.

Mr. Glaisher read a paper by *Mr. Pratt* of Brighton, on the belts of Saturn, in reference to the paper read at the previous Meeting by Mr. Ranyard. He believed that he had observed the belt described by Mr. Ranyard on several occasions recently, and that it was a feature which had been well known to him for several years. The following was the general arrangement of the belts on the planet, which were usually similar and symmetrical for the two hemispheres. On each side of the equator was a zone of a creamy-yellow tint, usually free from markings. At about lat. 10° there was a strong narrow belt, sharply defined on its equatoreal side, but diffused on its polar side, and gathered in places into wispy knots and curved markings. The colour of this was Vandyke-brown, and it was often the only detail visible. This appeared to be the belt under discussion. The polar side of this belt softened into a narrow cream-tint zone, which again merged into one of very pale rose-madder, ending in a fairly sharp boundary at about lat. 25°. Another cream-tint zone followed, and about lat. 40° a double belt of very pale rose-madder began; this was followed in turn by another cream-tint zone, until the bluish-white polar cap was reached.

Mr. Ranyard. Since the last Meeting of the Society I have heard from Mr. Denning that he, like Mr. Pratt, had seen this belt last year. He has also seen it during the last few days, and has sent me a drawing of it. We both place it a little further south than 10° south latitude, perhaps as far as 15°, and Mr. Denning represents it as nearly touching the more northern belts. Mr. Ward, of Belfast, has also seen the same marking, so that there can be very little doubt that there really is a narrow belt in that position. I myself suspected (and Mr. Green has told me he has seen them) the existence of loops somewhat resembling the markings on Jupiter, but not, like them, egg-shaped. They are rather shallow notches on the upperside of the belt, and are much more marked in the photograph of Mr. Pratt's drawing than I have seen them. With regard to Mr. Pratt's description of the colours of the belts, there must always be great differences in the estimations of such faint tints, but the belt appeared to me as bluish grey rather than red.

Lieut.-Col. Herschel. I think there can be no doubt about the existence of this belt. My brother has told me that he had seen a belt of that character, and that his daughter also saw it when he pointed it out to her.

Mr. Knobel. The belt drawn by Mr. Pratt is almost identical with the belt of Trouvelot in his well-known drawing of Saturn, published in the Proceedings of the American Academy. It shows these loops or condensations of darkness on the belt.

Captain Noble. If I understand the matter, the belt shown in Mr. Pratt's drawing would now be in contact with the ring, for, owing to the change in the position of Saturn, we now see the other side of the ring to that which was presented to us in 1874, when this drawing was made; this notched belt, therefore, if it be still in existence, would probably be confused with the crape-ring where it crosses the ball of the planet.

Mr. Ranyard. I perceive that what I saw is not exactly what Mr. Trouvelot represents. The belt he represents is rather a series of clouds arranged in a line than a distinct narrow belt. Mine was a narrow belt with slight irregularities in it.

Capt. Noble. I have seen something in contact with ring B this year, which may have been Mr. Ranyard's belt, but which I supposed to be the shadow of the ring on the planet. It was in contact with the edge of the crape-ring where it crossed the ball, and was a dark line following the same elliptical curvature as the inner edge of the ring.

Mr. Green made a sketch on the black-board to indicate the relative positions of the different belts on Saturn. Mr. Green said that the ring hid the northern part of the planet, but the outline of the ball could be traced on either side for some little distance through ring C, the dusky ring. To the south of the dusky ring was a bright belt, the central portion of which was so brilliant as to almost suggest that Mr. Brett's hypothesis of specular reflection might be applicable to it. Further to the south there was a broad dark belt, which he thought was the one Mr. Pratt had described as the Vandyke-brown belt, which he should be more inclined to speak of as a sepia belt. In contact with this was a narrow belt, which he believed was the one Mr. Ranyard had described. It had the looped edges referred to, and was not a clean even belt. The colour of the dark belts seemed to him of a rich yellowish brown, something of the tint seen on a Cochin-China egg. Further to the south was another belt, and finally there was a faint ill-defined cap round the southern pole.

Mr. Ranyard said that he perfectly agreed with Mr. Green as to the position of the looped belt.

Mr. Green stated that he had seen it very clearly last January. The brown colour had then been very much more diffused, but there had been great concentration since the December opposition of last year. There had been great changes on the planet between the early part of last year and the planet's coming to a favourable position again in the latter part of the year. Referring to another matter, Mr. Green stated that when Cassini's division between the inner and outer rings was just clear of the south pole of the planet, he had seen very distinctly what he had noticed on other similar occasions, but never so marked, that the shadow from the ball itself seemed to make a little horn where it fell on ring B close to Cassini's division. So far as he knew, no mention of this appearance had appeared. He had also remarked what had been mentioned in several publications, that the appearance of a division in the outer ring, which existed during imperfect definition, was lost when the definition was good.

Mr. Ranyard stated that Professor Young had recently informed him that, in company with Professor Harkness, he had seen the ball of the planet through Cassini's division with the Washington 26-inch refractor. They had seen the division to be perceptibly less dark where the ball of the planet was behind it. Probably this was the first time that it had been noticed.

Mr. De La Rue. I may say with regard to the division in the outer ring, to which Mr. Green has alluded, that I have represented it in engravings published some years ago. I have seen it distinctly black; if it is not so distinct now, that may be due to changes which have taken place since*.

Mr. Sadler. I think there can be no doubt about the existence of this division, for it has been measured by Struvé.

Mr. Brett. Is Professor Young's observation recorded?

Mr. Ranyard. I have only heard of it in his letter last week.

The Chairman. I am sure you will all return thanks to Mr. Pratt for having elicited this interesting discussion of a question

* [March, 1858, for example.—*W. De La Rue.*]

on which amateur observers are so well qualified to contribute valuable information.

Mr. Knobel read some extracts from an important communication from *Mr. D. Gill*, respecting his "Heliometer Determinations of Stellar Parallax." It contained the results of observations of several stars made separately, and in conjunction, by Mr. Gill, Dr. Elkin, and others. The distance of each star, the parallax of which was sought to be determined, from two comparison stars situated on opposite sides of it was measured at the times when the effect of parallax was least and when it was greatest. The following were the results obtained for the stars observed:—

	Parallax.	Probable Error.
	"	"
α Centauri	+0·75	±0·01
Sirius	+0·38	± ·01
ε Indi	+0·22	± ·03
Lacaille 9352	+0·28	± ·02
O_2 Eridani	+0·166	± ·018
β Centauri	−0·018	± ·019
ζ Toucani	+0·06	± ·019
e Eridani	+0·14	± ·020

The probable error of a single observation by Mr. Gill averaged 0"·1, and for a single observation of Dr. Elkin, 0"·16. The determinations had all been made with the Cape heliometer of 4 inches aperture, and with a power of 175 diameters.

Mr. Gill referred to the importance of parallax investigations, in order that our knowledge of the sidereal system might be advanced. We did not know at present whether bright stars or stars having large proper motions were the more likely to give large parallaxes. There were therefore two questions to be solved:— First, what is the average parallax of stars of the first magnitude, of stars of the second magnitude, of the third, and so on [Hear, hear]? and second, what connection is there between the parallaxes of the stars and their proper motions? The present series of measures showed that the parallax of a star could be determined from 16 measures, with an average probable error of 0"·02, assuming that the observations were free from systematic errors; and with a more powerful instrument, which would give a greater choice of comparison stars, it would seem that any systematic errors might be eliminated. There were 16 stars of the first magnitude in the southern heavens; a similar number of stars might be selected of the second, 16 more of the third, and so on. In making these observations a reversing prism should always be employed, as in the Cape measures, that the results might not be affected by the position of the comparison stars. It should also be always borne in mind that measures of two or more pairs of stars were much better than repeated measures of the same pair of comparison stars. Another most necessary precaution was the use of screens, to render the two stars equal in brightness. The heliometer employed should have a considerably greater light-gathering power than the Cape instrument, that there might be a freer choice of comparison stars. It should be at least of 7 inches aperture. A considerably higher power than the one used in the Cape determinations should also be employed. A single observer, by making 200 or 250 observations each year, might complete the entire series in the course of 10 years. This was a work urgently demanded in the interest of sidereal astronomy, and one that should be undertaken without hesitation or delay. (Hear, hear.)

Mr. Bryant asked what was the meaning of a *minus* parallax?

The Chairman. It means that the parallax of the principal star is less than that of the comparison star, and is presumably very small. It must be remembered that these are relative parallaxes, *i.e.* differences between the parallax of the principal star and of the comparison stars. In the case of α Centauri (for which there were four different pairs of stars compared with the principal star) the result is not, strictly speaking, inconsistent with the larger absolute parallax found from meridian observations. The parallax of Sirus, found by Mr. Gill and Dr. Elkin, is rather larger than I think has been found by other observers.

Capt. Noble. Are not the comparison stars supposed to have no parallax, or, in other words, to be infinitely distant?

The Chairman. The results really give the difference of parallax between the star observed and the comparison stars. It is practically assumed that the parallax of the stars of comparison is insensible. In the case of β Centauri it should be distinctly understood that the result here given is the relative parallax, whereas the absolute parallax was found by Henderson and Maclear in the cases of α Centauri and β Centauri, but the results are exposed to comparatively large probable errors.

Lt.-Col. Tupman. Dr. Gill used comparison stars at great distances I believe. I should like to know about how great is the maximum distance he employed?

The Chairman. He has 3830″ in the case of α Centauri, in another case 6000″ and 5460″; in another 3680″ and 3630″; and in the case of ϵ Indi 2640″.

Lt.-Col. Tupman. And Mr. Gill pretends to measure these long distances to within 0″·1! (Laughter.)

Mr. Sadler. O_2 Eridani has a very large proper motion.

The Chairman. It seems to me that Mr. Gill has attacked this question of stellar parallax in a particularly broad way. He has laid out a scheme which I hope he may have strength and energy to carry out, and it will go far to settle that important question, whether the nearer stars are those which have a large proper motion or those which are brighter. There seems to be some difference of opinion on that point at present; at least, the *data* are uncertain. I am sure you will be glad to hear that Mr. Gill is now on his way to England, and we expect him here at the February Meeting, when we shall be able to get more detailed information as to his method of observation, and as to the precautions he has taken in measuring these long distances of 3000″ and 4000″. I think, from the care and skill that he showed in his Mars observations, that we may trust that he would be fully alive to the errors to which these observations are liable, and that he will have taken every known precaution to eliminate these errors. The suggestion that Mr. Gill makes as to the employment of a large heliometer for this work is a very important one in the present aspect of sidereal astronomy; for speculations as to distances, magnitudes, and proper motions are altogether out of place until we have some further data to go upon.

Mr. Hilger exhibited an improved method of illuminating the wires of a micrometer. He had exhibited a form of micrometer at the Meeting of the Society in June last, in which the wires had been illuminated by a vacuum-tube placed behind them. It had then been suggested by the Astronomer Royal that it would be better if the vacuum-tube could be placed in front of the wires, between them and the eyepiece. This he had been able to accomplish after some difficulty, and had now obtained a beautiful and uniform illumination over every part of the wires, an illumination which was indeed much more satisfactory even than when the wires were illuminated from behind, since there had then been a slight lighting up of the field, but now the wires were bright in an absolutely dark field, in which even a faint star could be easily seen. The vacuum-tube, which was bent into a complete circle, was $\frac{1}{10}$ inch in diameter, and the bore was $\frac{1}{200}$ inch. He had found it very easy to illuminate the ruled plate of glass about which Prof. Pritchard had asked him.

Mr. Common observed that he had given much attention to this subject, and had tried a similar plan, but had lighted the wires from one side. He had mentioned on a former occasion that the great trouble was the liability to get a shock in the eye (Laughter.) He noticed in Mr. Hilger's instrument that if the micrometer were rotated the wires would get in the way. He pointed out that this might be obviated if the terminals of the vacuum-tube were led up into the fixed tube, and brought to bear on silver or platinum rings. That would make all the difference between the success and failure of the instrument. In lighting up a micrometer by means of a vacuum-tube, there might also sometimes be a difficulty in getting the coil to work. For this reason he had resorted to the old oil-lamp, which was always ready for application and easy to use.

The Chairman. I am glad to find that a small criticism I made has led Mr. Hilger to make an improvement in his micrometer, and I hope Mr. Common's criticism will also lead to a yet further improvement.

The Astronomer Royal. I have a paper on "The Spectroscopic Results for the Motions of Stars in the Line of Sight." Mr. Maunder and Mr. Nash have been continuing at Greenwich the spectroscopic determination of the motions of the stars, and during the past year 45 stars have been observed, and the results are given in tables here. There is a great difference in the accuracy with which different stars can be observed, depending on the character of the lines in their spectra as well as on their brightness.

We are working tentatively at present, and we are taking all the stars to which we can apply the method. Until we try a star we can hardly tell whether it will give a trustworthy result. I would point out that while the observations of Arcturus and Vega show consistent results throughout, there are other stars which give discordant results; therefore the results for these latter stars must be taken *cum grano*, and we only bring them forward for what they are worth. There is one very interesting case we are now discussing, and that is as to the motion of Sirius. We have found in the last two or three years that the motion of Sirius, instead of being a motion of recession of 18 or 20 miles a second, has appeared to be changing into a motion of approach. But that is a point that requires a great deal of confirmation.

It must be understood that the displacements we are measuring are very small, although they may appear to be large when expressed in miles per second. The suspected change in the motion of Sirius might be explained by an orbital motion of the star; but it is altogether of a different order of magnitude to the known orbital motion in a plane perpendicular to the line of sight, and would not be accounted for by that.

Mr. Downing. With regard to the effect of orbital motion on motion in the line of sight, it would be advantageous if the spectroscope could be applied in some cases to the orbits of the binary stars. α Centauri is perhaps the most favourable case, since the plane of the orbit is inclined at an angle of nearly 80° to the plane on which we see it projected, so that in one part of its path the *comes* is relatively moving almost directly towards us, and in the opposite part almost directly away from us. It would be interesting if the relative motion of approach and recession could be measured with the spectroscope. Of course there is nothing novel in the suggestion to make such observations, but, as far as I know, they have never yet been made. If some one in the southern hemisphere possessed an instrument of sufficient power, it would be interesting to apply it to α Centauri, and see if the approach and recession could be measured.

Lieut.-Col. Herschel. Is there no star in the northern hemisphere available?

The Astronomer Royal. I think not. The suggestion of Mr. Downing appears to be a valuable one if any observer in the southern hemisphere could be induced to take it up. α Centauri is, I believe, the only star that is favourably circumstanced in every respect for this observation.

Mr. Inwards explained, by means of a moving model, a new chair or observing-seat, which he had devised for use with the Transit instrument. He pointed out that as the eyepiece of the telescope was brought down, and the chair was brought forward, the seat became depressed as the back increased in slope, the reverse action occurring when the chair was pushed back, so that the observer's head and back were always supported in a comfortable position.

Mr. Green obtained leave to make a statement with reference to the recent remarkable sunset-glows, of which he had seen a very beautiful example that evening. He pointed out that the peculiar feature of these glows consisted in the appearance of an arch of ruddy light, high up in the sky, some considerable time after the ordinary sunset-tints had disappeared. He would like to ask if any difference had been noticed in refraction during the period of these glows. If not, we cannot say they are a question of refraction, since increased refraction would cause some change, however slight, in the position of stars near the horizon.

Admiral Sir Erasmus Ommanney said that he had repeatedly noticed the red arc described by Mr. Green. It was usually visible sinking slowly towards the horizon for some 40 or 45 minutes after the Sun had set. These sunsets reminded him of the appearances he had seen in the Arctic regions when the Sun was about to disappear for the winter.

Mr. Brett asked in what these sunsets differed from ordinary sunsets.

Mr. Green replied that instead of the colour becoming richer towards the Sun as was usually the case, there was a distinct arch of glowing light away from the Sun.

Mr. Ranyard said that it seemed to him that these arcs of light were probably due to refraction of light by ice-crystals at a greater height than usual, caused by an abnormal amount of dust in the air; indeed both causes might be acting. The spectroscope showed a dry air-band of abnormal intensity, and at the same time evidence of an abnormal amount of water vapour.

Mr. Chambers stated that a civil engineer, in charge of some large works at Eastbourne for crushing sea-beach to make fine sand, had described to him some curious optical effects produced when the Sun was viewed through the very fine dust which arose from the operations he had in progress, and which seemed to confirm, to some extent, Mr. Lockyer's views, that these effects were produced by volcanic dust.

Mr. Mattieu Williams dissented from Admiral Ommanney's opinion that these sunsets resembled Arctic sunsets. Arctic sunsets only differed from our own in their prolonged duration. But we had recently had, not only a redness connected with the Sun, it had been seen round the Moon, at some 8° or 12° from it. Some of the after-glows had not terminated for an hour and a quarter after sunset, and that in September, when the Sun went down almost perpendicularly.

Mr. Rand Capron mentioned that a gentleman at Lancaster, Mr. Clapham, had seen a peculiar pink tint round the Sun on the blue sky, the Sun being on the meridian. He also fully corroborated Mr. Ranyard's statements as to the recent abnormal intensity of the rain-band. There was also a strong dry air-band near D, showing that there were two strata—a very moist stratum of air probably close to the earth, and a dry one, probably much higher up. His hygrometer readings had also showed the atmosphere to be in a peculiarly moist condition during the month of November.

The following papers were announced:—

W. H. Finlay. "Observations of Comet III. 1882, Barnard."

W. F. Denning. "The Meteor-shower of Pons's Comet."

Communicated by the Astronomer Royal. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, observed at the Royal Observatory, Greenwich, in the year 1883."

E. J. Stone. "Note giving a numerical Illustration of the Effects of a percentage change in the Sun's mean Motion in Longitude."

R. Bryant. "Elements of the Orbit of Comet a 1883 (Brooks-Swift)."

The following gentlemen were duly elected Fellows of the Society:—W. Henry Davies, Edward H. Nightingale, Cuthbert E. Peek, B.A., and Rev. W. H. K. Soames, M.A.

The Meeting adjourned at $10^h\ 0^m$.

Meeting of the Liverpool Astronomical Society.

The fourth Meeting of the Session was held on Monday evening, 21 January. The Rev. T. E. Espin, B.A., F.R.A.S. (Vice-President), in the Chair. Fourteen Members were elected and Mr. R. A. Proctor, M.A., F.R.A.S., Captain William Noble, F.R.A.S., and M. François Terby, D.Sc., F.R.A.S., were elected Associates of the Society.

The Secretary read the following paper by the Astronomer Royal for Scotland, on "95 Herculis, and the problem of its colours."

"In the Abstract of Proceedings of the Liverpool Astronomical Society for Dec. 17, 1883, there is a short paper on the colours of the double star 95 Herculis, which tends to show that one component of that star is always of a light green, and the other of a light cherry or cerise colour; while the far more strongly contrasted colours attributed to them by certain careful observers are supposed to have arisen out of a confusion of nomenclature.

"But pray permit me to ask, how can that logically explain any case when there is no contrast at all? as, according to that classic observer, William Struvé, they were at one date both grey.

"I can readily grant to the Liverpool Society that, *generally*, one star is no more than slightly green and the other slightly red. But, if the Society has also established that on a certain occasion they have both been seen of one and the same colour, it opens up a probability that on some other occasion they may have been more diversely coloured than in their mean and average condition. Hence, if the Society proposes to establish a permanent set of star-tints, may I express a hope that they will attach a date to every observation they may make of so undoubtedly a variably coloured star as 95 Herculis?

"But more than that, will not so young and vigorous a Society trace up star-colours to their origin, or at least to their places in the spectrum? It was well enough for observers of past generations to describe colours superficially; but spectrum analysis has shown us that we may gain as perfect a knowledge of the chemical constitutions of stars as a chemist can of the identity of a metallic salt on his laboratory table."

The Chairman said he thought Prof. Smyth had attached too much importance to what was, after all, but the expression of opinion from an individual member. The Society was glad to encourage observational astronomy, and to publish the results of such observations, but they ought not to be held responsible for errors that might be contained in those observations. With the work done at the observatory it was different, and he thought the minutest detail of time, magnitude, and colour was there attended to. With regard to tracing the colours of stars up to their places in the spectrum, that might hereafter be done, but at present it was impossible, as the amount of work entailed on the sections was something enormous. The suggestion had, however, reminded him that Dr. Huggins had kindly presented the Society with a complete series of his works, and had expressed his sympathy with the objects of what he was pleased to term "the infant Hercules." (Laughter.)

The Chairman then read a paper "On a new Theory of the cause of Stellar Variation, and a possible example of it in the case of 37 Pegasi."

The star 37 Pegasi is a known binary. At the time of its discovery the components were 1 second of arc apart; but in 1866 it was observed to be single by Secchi, since which time it had probably widened, as it was half a second apart in 1873, and Mr. Espin had himself observed it as strongly elongated, if not divided, in October 1880. Mr. Pocklington, while observing stars at his request in this region during the autumn of 1883, had found the magnitude 5·3. In the 'Uranometria Argentina' its magnitude was 5·8, though in the two neighbouring stars, 34 and 35 Pegasi, Mr. Pocklington and Dr. Gould quite agreed. This result was so very curious, that Mr. Espin measured 37 with the photometer on Dec. 6, and the magnitude came out 5·4. It at once suggested itself to him that the variation in its light might be due to the closing and widening of the two stars. He believed that this might account for the long periods of fluctuation in many stars, and, if so, it afforded a new method of determining which were close double-stars, even if they could not be separated by the telescope.

A paper on the Meteors of February 20, by Mr. W. F. Denning, F.R.A.S., was read. During the night of Feb. 20, 1877, he had observed two well-defined meteoric showers from the points

R.A. 181°, Dec. 34° N.
R.A. 263 , Dec. 36 N.

The meteors were rather bright and moved in swift courses. Mr. Denning was desirous of calling attention to these streams, in the hope that they might be observed during the present year.

As to the former shower, situated N. of Coma Berenices, it appears to have been seen by several other observers, as below:—

	R.A.	Dec.	
Jan. 1–25	183°	+36°	Greg and Herschel.
Jan. 4–11	180	+35	Tupman.
Feb. 3–10	180	+35	Tupman.
Feb. 6..........	187	+31	Schiaparelli and Zezioli.
Jan.–March 12 ..	179	$+35\frac{1}{2}$	Italian obs., 1872.

It is obviously one of those radiants which, though of long endurance, yet display special activity on certain nights.

In a paper on the occultation of α^2 Libræ on 6th Sept. last, Mr. E. F. Sawyer stated that at Cambridgeport, U.S.A., the occultation was successfully observed by several members of the Boston Society. He had been greatly surprised to notice the star gradually projecting itself on the dark limb of the Moon until it had very nearly, if not wholly, advanced inside the dark edge, when, without any gradual diminution of light, it suddenly disappeared. Papers were also read from Mr. W. S. Franks, F.R.A.S., and Mr. W. Goodacre; and the meeting adjourned at $9^h\ 30^m$.

The Biela and Pons Meteors, 1883.

Although short watches at intervals were taken on the 25th, 27th, 28th, and 29th of Nov., no traces of the Biela meteor-stream were recorded.

Short watches were also taken on the evenings of Dec. 3, 4, 6, and 7 for possible traces of the meteors belonging to Pons's 1812 Comet, but only two meteors were observed and mapped belonging to this cometary meteor-stream, and these were recorded on the 6th—one at $6^h\ 10^m$ from R.A. 290° +64° to $297\frac{1}{2}°$ $+51\frac{1}{2}°$, and the other at $7^h\ 51^m$ from RA. 264° $+57\frac{1}{2}°$ to 270° +62°. They were both of the 4th mag. and were rapid in their flight. These were observed during the early evening and during the moonlight. I understand, however, that Mr. O. C. Wendell, of the Harvard College Observatory, was more successful, and, observing during the latter part of the night and in the absence of the moonlight, he mapped several meteors belonging to this stream, the results of which will no doubt soon be published.

Edwin F. Sawyer.

Cambridgeport,
1883, Dec. 16.

The Rotation-period of Mercury and other Planets of the Solar System.

CERTAIN markings* which I discerned on Mercury in the mornings of November 1882 led me to conclude that the rotation-period of this planet is not accurately given in our text-books. I judged the true period would be about 25 hours; but the observations on which this estimate depends covered too short an interval to give a reliable period. If this result is, however, approximately correct, the rotation of about 24 hours derived from the combined observations of Schroeter and Harding, of Lilienthal, at the end of the last and opening of the present century, falls considerably short of the actual value.

Signor Schiaparelli has been very successful during the last two years in observing definite markings on Mercury, and he has written me in confirmation of the suspicion that the rotation-period as usually adopted is not exact, in fact "very far from the truth." But I have not learnt whether Sig. Schiaparelli finds the period greater or less than that of Schroeter. His forthcoming memoir on Mercury will doubtless supply the information, and give us some very interesting details of the physical aspect of this planet. In the meantime I trust observers will endeavour to obtain some clue to the visible phenomena on the surface of Mercury. Considering the unfavourable circumstances attending observations of this planet, it is not to be wondered at that we know little or nothing of its surface phenomena, but it is obvious that the markings are of a fairly distinct character and that this object is likely to prove a very fruitful one for further investigation. The general aspect of the disk as I saw it in Nov. 1882 reminded me forcibly of Mars; the definite nature of the spots may therefore be readily imagined. As to whether or not they are permanent is a problem which must be solved by persistent observation in the future; but from what I have seen I conclude the dark spots and shadings may be regarded as fairly permanent, while the white spots are influenced by rapid variations.

On comparing the different rotation-periods of the planets the idea is suggested of a dominant law controlling this element. The axial motion is swifter with increased size. This general fact is clearly evident, though it does not admit of critical investigation, inasmuch as we are not cognizant of the *exact* periods of any of the planets besides the Earth and Mars. Jupiter's rotation is unknown, owing to the existence of dense envelopes obscuring the surface, and Saturn is very probably affected in a similar manner. The white spot distinguished on the latter planet in Dec. 1876 was evidently a mere temporary formation in the outer envelope

* One of these, observed on Nov. 9 at sunrise, was very interesting. There was a bright area between the E.N.E. limb and the terminator, in the centre of which lay a very brilliant small spot, with luminous veins or radiations extending over the whole area. Several very definite dark spots were also visible on the disk.

of Saturn and not a material portion of the surface, for after a few weeks' visibility it eventually extended itself on one side into a streak, thus evincing a similar disposition to certain markings on Jupiter. We must therefore conclude that this white spot with its period of $10^h\ 14^m\ 23^s{\cdot}8$ simply represented the velocity of an atmospheric current on the planet and cannot be held to have indicated the true period of rotation. But the times derived from the markings both on Saturn and Jupiter, though not corresponding to the actual rotation of those planets*, are yet useful as furnishing the clue to an approximate determination of their true periods. The prevailing law of rotation imparts an immensely greater velocity to the larger planets of our system than to the smaller, and there may be some element other than simple relative dimensions operating in this effect, though the difference in size evidently exercises the primary influence. As to Mercury and Venus, it must be confessed that their times are involved in doubt, owing to the fact that the latter only shows markings of great feebleness and the former is rarely observed on account of solar proximity. But if the inferior planets rotate in periods varying in the same ratio as the Earth and Mars, then the length of a day on Venus would be 24^h and on Mercury rather more than 25^h. Of course, if we admit the period of $23^h\ 21^m$ found for Venus by Cassini in 1666 and 1667 and confirmed by Schroeter (1789), De Vico (1839–41), and others, the rule falls to the ground; but we must remember that Schroeter in 1801 derived a rotation-period of 24^h from a faint dusky streak visible on that planet.

The possessors of large instruments will now be doing good service by attacking Uranus with the idea of observing the surface-markings for rotation. Prof. Young using the 23-in. equatoreal of the Princetown, N. Y., Observatory, found two faint belts always visible on each side of the equator in May and June 1883; and Prof. Schiaparelli, with the 8-in. Merz refractor of the Milan Observatory, has ascertained that there are spots and differences of colour visible on the disk of Uranus, "but not sufficiently distinct to be followed in outline with this instrument." On March 2, 1883, 13^h, in very clear air, I saw a light zone crossing the planet slightly S. of the centre and bounded by two extremely faint and delicate bands like the fainter belts on Saturn. On other occasions I traced objects of like nature, though the light of my 10-inch reflector appeared insufficient to grapple successfully with such feeble markings.

* It seems desirable therefore that the definite statement of the periods of these planets should not be made in astronomical works without suitable qualification. To say that the planet Jupiter rotates in $9^h\ 55^m\ 36^s{\cdot}2$, because that is the mean period of more than 5 years' observation of the red spot, would be an assumption not warranted by the facts, and one likely to hide the truth, for the spot is probably quite detached from the surface and has possibly a very different period. It is therefore necessary always to regard the proper motion of the spots on Jupiter and Saturn and the fact that the actual surface of these planets is never certainly exposed to our examination.

As to the rotation-period of Saturn, it is well known that the erroneous period of $10^h\ 29^m\ 16^s{\cdot}8$ instead of $10^h\ 16^m$ was long understood to be Sir W. Herschel's value. The more recent determination by Prof. A. Hall will, it is hoped, be more correctly transmitted to posterity. In Prof. Ball's 'Elements of Astronomy,' p. 404, the latter period is given as $10^h\ 14^m\ 28^s{\cdot}8$, and in Mr. Webb's 'Celestial Objects,' p. 175, as $10^h\ 15^m$, while the 'Monthly Notices,' vol. xxxviii. p. 210, mentions the period as $10^h\ 14^m\ 23^s{\cdot}8$. I presume the latter is the correct value?

Bristol, 1884, January. W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

On the Definition of Mean Solar Time.

Sir,—

The report contained in this month's 'Observatory' of the remarks which I made on Mr. Stone's theory at the December Meeting of the Royal Astronomical Society is very imperfect. Unfortunately, I had no opportunity of correcting the press before it appeared. I shall therefore be obliged if you will insert in your next number the following remarks, which contain in, I hope, a more intelligible form the substance of what I said at the meeting.

Cambridge Observatory, 1884, Jan. 2. Yours faithfully, J. C. Adams.

Remarks on Mr. Stone's Theory of Mean Solar Time.

The proper definition of Mean Solar time appears to me to be a very simple matter, and to have nothing arbitrary about it. The mean Sun is merely an imaginary body which is supposed to move uniformly in the equator at such a rate that the difference between its right ascension at any time and that of the true Sun consists entirely of periodic terms. This difference is called the Equation of Time, which therefore, by its very nature, cannot contain any term increasing indefinitely with the time. Mean noon at any place is determined by the transit of this imaginary body over the meridian of the place, just as apparent noon is determined by the transit of the true Sun.

Thus mean time is defined with reference to a natural phenomenon, viz. the transit of the real Sun over a given meridian; and we cannot have one length of a mean solar day according to Bessel, and another length according to Le Verrier, any more than we can have different lengths of the apparent solar day.

A mean solar day, according to Mr. Stone's theory, is something totally different from that above defined. It has no reference to the average length of the apparent solar day, but is purely artificial or conventional in character. Practically, Mr. Stone's mean solar day is the time during which the mean *longitude* of the Sun increases by some definite amount. Bessel gives one determination of this amount, and Le Verrier a different one. Hence Mr. Stone is obliged to employ two mean solar days, which are of different lengths according as Bessel's or Le Verrier's mean motion of the Sun is used. On this principle, every fresh investigator of the Sun's motion would require a mean solar day peculiar to himself! We are tempted to ask, what was the meaning of the mean solar day before Bessel's time?

The origin of Mr. Stone's misapprehension on this point seems to be the following:—In the ordinary practice of an observatory it is usual and convenient to deduce the mean solar time from the sidereal time supposed to be known, instead of finding it by direct observation of the Sun. In order that this conversion of sidereal into mean solar time, however, may be correctly performed, it is necessary to employ the correct mean longitude of the Sun at the given instant. Any error in the assumed mean longitude will produce an equivalent error in the mean time deduced, and if the Sun's mean motion be incorrectly assumed, the error of time thus produced will gradually accumulate.

Thus the error of mean solar time as deduced from sidereal time by means of Bessel's formula, which amounted in the year 1864 to a little more than half a second, has increased to a little more than six tenths of a second at the present time. The increase of the error of mean solar time in 19 years is in reality rather less than 8 hundredths of a second, whereas Mr. Stone's theory makes it amount to 27 seconds! In fact the error according to Mr. Stone's theory is about 365 times as great as it should be. The reason is that mean time is measured, *not* by the Sun's mean motion in *longitude*, as Mr. Stone's theory supposes, but by its mean motion in *hour-angle*, which is about 365 times as great, so that the error in time produced by a small error in the mean motion in longitude is only about $\frac{1}{365}$th of that which would be produced if the error in time bore the same proportion to the time that the error in the mean motion in longitude bears to this mean motion itself.

If n denote the Sun's mean motion in longitude in a mean solar day, then the ratio of the length of a mean solar to that of a sidereal day is

$$360^\circ + n : 360^\circ.$$

And if $n+\delta n$ denote a slightly different determination of the mean motion in longitude, this ratio will be altered to

$$360 + n + \delta n : 360^\circ.$$

Hence the measure of the sidereal interval corresponding to any given number of mean solar days will be altered in the ratio of

$$360^\circ + n + \delta n : 360^\circ + n$$

or

$$1 + \frac{\delta n}{360^\circ + n} : 1;$$

that is, since $360°$ is nearly equal to $365n$, the sidereal measure of the interval will be altered nearly in the ratio of

$$1 + \frac{1}{366}\frac{\delta n}{n} : 1$$

instead of in the ratio of

$$1 + \frac{\delta n}{n} : 1$$

as it should be by Mr. Stone's theory.

In conclusion, we will test Mr. Stone's theory of mean solar time by supposing an extreme case. Let us imagine that the Sun had *no motion* in longitude, but, like a fixed star, retained a constant position in the heavens. On this supposition, mean solar time would be just as intelligible as it is at present, and it is evident that the mean solar day and the sidereal day would become identical with each other; but what would become of mean solar time according to Mr. Stone's idea of it?

The so-called New Star of A.D. 389.

SIR,—

The P.S. of my letter in your last number (p. 18) was printed, as well as written, rather hurriedly, and the name of Cuspinianus (the Latinized form of Spieshammer) is unfortunately given as "Crespinianus." But my principal object in writing to you now is to mention that I have found that the mistake about the so-called new "star" of A.D. 389 was made before Humboldt by Cassini, from whence the error in the 'Kosmos' was therefore probably copied. It is to be found at p. 59 of his 'Eléments d'Astronomie' (published in 1740), where, speaking of new stars, he says:—

"Une troisième que Cuspinianus, au rapport de Licetus (p. 259) découvrit l'an 389 vers l'Aigle, et qui cessa de paroître, après avoir été vûe aussi brillante que Venus, dans l'espace de trois semaines."

This of course clearly implies that Cuspinianus had himself seen the star; but I should imagine that the fact mentioned in the last sentence of the P.S. of my former letter will be admitted to be a complete proof that this could not have been the case.

The question immediately arises, was the mistake made by Cassini or by Licetus (or rather Liceti), from whom he quotes? To answer this, I have turned to his book, 'De Novis Astris et Cometis' (published at Venice in 1622), and at page 259 the whole reference to Cuspinianus is contained in the passage:—"Cuspinianus autem paullo post, nimirum anno a nativitate Domini tercentesimo octagesimo nono, ut retulit etiam Tycho, stellam quamdam a Septemtrione circa Gallicinium scribit ascendisse, et instar Luciferi splenduisse, atque intra spatium trium hebdomadarum disparuisse."

Licetus, therefore, merely states that Cuspinianus had narrated the fact of the appearance of this "star," and appeals to Tycho Brahe as having made the same assertion. I have referred, however, to the great work of Cuspinianus, 'De Cæsaribus atque Imperatoribus Romanis' (first published at Strassburg in 1540, eleven years after the author's death), and cannot find any mention of such a celestial appearance. Possibly it may be contained in some unpublished work of his; there is no other extant in which it would be likely to be. But the description of the "star" is evidently taken from that of Marcellinus, to which I referred in my last letter, where I have shown that it was in all probability the object mentioned by Philostorgius, which, from its motion amongst the stars, must have been a comet.

Allow me to add a few words respecting the name of the star now known as α Aquilæ or Altair, lest I may have led to a wrong impression about it. Ptolemy does often call the star itself *par excellence* by the name of the constellation, 'Aετος, or the Eagle. Ulugh Beigh, in the fifteenth century, calls it in Arabic "El-nesr el-tâïr," or the Flying Eagle (literally "the eagle, the flying"), which has been subsequently shortened into Altair (=the flying). As I have pointed out, Aquila could not have been in the position where the "star" is said to have been at the time in question; the mistake of assigning its place to that constellation when first seen would seem to have been made by Cassini.

Yours faithfully,
Blackheath, 1884, Jan. 3. W. T. Lynn.

The Sun-glow.

Sir,—

Among the flood of letters written within the last few weeks about the curious *solar-* or *after-glow* I have not seen one which alludes to the strange phenomenon being visible at mid-day, as has often been the case here, in fact on any day when that part of the sky near the Sun was free from cloud. On the 1st Dec. an astronomical friend and myself noticed it very distinctly, and it was still more marked on the 16th, when it presented the appearance of a beautifully delicate rosy halo, the inner edge of which seemed to be about 5° from the Sun, the glow extending some 20° to 25° outwards.

Anstwick Hall, Yours truly,
Clapham, Lancaster, 1884, Jan. 1. T. R. Clapham.

A Green Moon.

Sir,—

The most interesting phenomenon observed in connection with the recent remarkable sunsets as seen in this climate has undoubtedly been that of the "green" Moon; and as there appears to be a difference of opinion respecting the subjective or objective nature of the phenomenon, the following observations, as bearing upon the question, may be of sufficient interest to justify their publication.

January 6. After gazing at the "after-glow," which at this time, 4^h 40^m P.M., was very brilliant, I turned my attention to the Moon, which was shining in a cloudless portion of the sky, and I was surprised to find it of a very pale green colour; but after looking at it a few moments the green colour disappeared, and the Moon presented its normal appearance. Again directing my attention to the "after-glow," and observing it for a few moments, I once more looked at the Moon, which again appeared "greenish," and, as in the former instance, the colour disappeared after looking at it a few moments.

And thus, so long as the "after-glow" remained sufficiently brilliant, the "green" colour could be given to the Moon by first simply looking at the crimson and pink "after-glow" and immediately transferring the gaze to the Moon.

January 11, 5^h 30^m P.M. I again noticed that by first looking at the "after-glow" and looking at the Moon immediately after, it could be made to appear of a very pale greenish tint: but only for two or three seconds, as the moon being 13·2 days old was much too bright for the phenomenon to be so well observed as on the first occasion.

In connection with the foregoing observations it would be very interesting to know if those observers who have had the good fortune to observe this phenomenon, noticed the greenish colour of the Moon previous to their looking at the "glow"; or whether, like myself, it was not till after observing the "after-glow" that the Moon appeared of that colour.

From my own observations, and from the descriptions that have as yet been published relating to this phenomenon, I am decidedly of the opinion that the phenomenon is entirely subjective, being akin to the well-known optical experiment, in which a red object being looked at for 30 seconds, and the gaze then directed to a white surface for another 30 seconds, an image of the object appears projected on the white surface as a bluish-green counterpart of the red original. In this case the "after-glow" is the red object, and the Moon the white surface.

Yours faithfully,

Dalston, E., 1884, Jan. 14. B. J. HOPKINS.

NOTES.

THE SPECTRUM OF COMET 1883 *b* (PONS-BROOKS)*.—The spectrum of this comet has been examined at several different observatories, and the three usual cometary bands have been detected, the measures of which, as made at Greenwich Observatory and by M. Trépied at the Observatory of Algiers, show them to be coincident with the three principal bands in the spectrum of an alcohol flame of a Bunsen burner. M. Rayet of Bordeaux speaks

* Comptes Rendus, Vol. xcvii. Nos. 24 and 27, and Vol. xcviii. No. 1.

of the least refrangible band as being in the *orange*, and M. Trépied understands this to refer to the band between D and C; but it appears more likely that the band at λ 5633 is the one intended, though Sig. Ricco at Palermo states that he saw a band in the *red* with a Maclean spectroscope. A very narrow continuous spectrum from the nucleus was detected on Dec. 6 at Greenwich, and more recently a much fainter continuous spectrum from the nebulous district immediately surrounding the nucleus has been made out. M. Trépied on Dec. 27, and M. Thollon, have likewise recognized the continuous spectrum; but all observers agree in remarking on the great superiority of the band-spectrum to it in brightness. The spectrum of the comet is therefore chiefly gaseous, and M. Thollon points out its strong resemblance in this respect to Comet 1881 *c*. He also calls attention to the similarity in the form of the tails of the two comets; in each there was only a single band, instead of the usual two streamers with a dark rift behind the nucleus. In the comet of 1881, however, this shaded off about equally on both sides. In Pons's comet the southern edge is sharp, well-defined, and nearly straight, whilst the other side is nebulous and diffused. M. Thollon suggests that this form of tail may possibly be characteristic of comets in which the gaseous spectrum predominates. All observers speak of the great brightness of the middle of the three bands (λ5164); the band in the blue was exceedingly faint; the third band, the least refrangible, was much fainter than the green band, but was not so difficult an object as the one in the blue.

TRANSIT OF COMET 1883 *b* (PONS-BROOKS) OVER A STAR.—An observer in Jacksonville, Florida, reports:—"Early in the evening of Dec. 24 I found Pons' Comet close to a star in Cygnus of the seventh magnitude, and from its relative position could see, at a glance, that a transit was inevitable. As I had been for three months watching this very comet for such an opportunity, I hurried up my preparations, putting on a power of 100, and getting an assistant to mark time. The following are the results:—Star began to fade at $6^h\ 31^m\ 42^s$, local time; star began to brighten at $6^h\ 35^m\ 22^s$; interval, $3^m\ 40^s$; middle time of transit, $6^h\ 33^m\ 32^s$; longitude west of Greenwich, $5^h\ 26^m\ 28^s$; or (by a singular coincidence) at 12^h Greenwich mean midnight." The star faded by estimation half a magnitude. The observer could not state positively that the star and the centre of the nucleus precisely coincided, "but to all appearances the central line of passage was through the star; yet I found it impossible to see the stellar centre of the comet in contact with the star on either side, and there was an interval corresponding with the waning of the star's light, when the nucleus was invisible." The star referred to is Arg. Z. +32°, No. 3980, mag. 6·0.

Variations in the Light of Comet 1883 *b* (Pons-Brooks)*.—On the first day of the present year, the Pons-Brooks comet underwent a sudden change of brightness for the third time. Dr. Müller, of the Potsdam Observatory, observed the comet 1884, Jan. 1, $5^h\ 47^m$ Potsdam M.T., when its appearance and brightness accorded with those it had displayed on the preceding days. But at $7^h\ 20^m$, on looking at the comet again, he was surprised to note how great a change had taken place. Instead of the diffused nucleus he had seen an hour and a half earlier, there was now an almost stellar point of about the brightness of a 7 mag. star. Dr. Müller carefully watched the comet during the next two hours, and compared its light with that of two neighbouring stars, D.M. $+24^\circ$ 4471 and $+24^\circ$ 4473, estimated in the 'Durchmusterung' as 7·0 and 6·8, with the following results:—

At $7^h\ 28^m$	7·53 mag.	At $8^h\ 27^m$	7·03 mag.
7 41	7·35	8 38	7·00
7 58	6·97	9 0	7·13
8 7	6·89	9 7	7·33

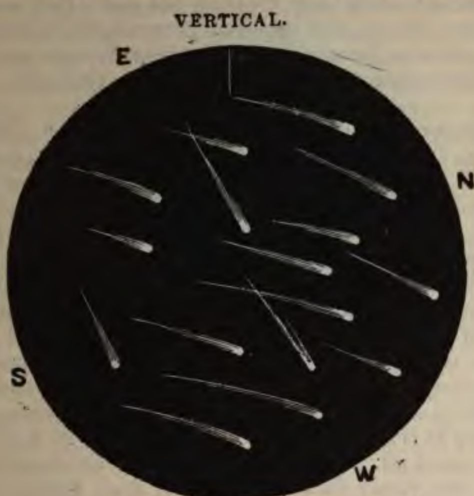
The greatest brightness would appear to have taken place about $8^h\ 12^m$ Potsdam M.T. or $7^h\ 20^m$ Greenwich M.T. At $9^h\ 30^m$, the comet had again changed its appearance, and resumed the form and brightness it had presented before this sudden change. The entire change of brightness was about equal to 1·3 mag.

Clark's Transit Tables for 1884†.—As this is the second year of publication and we noticed the first issue, we need not say more than that the book is chiefly intended as a companion to the small Transit Instruments which Mr. Latimer Clark has designed, but that it would prove very useful to any amateur astronomer who may possess a transit instrument of any kind. The introduction gives very clear instructions for the management of a small transit, expressed in simple and untechnical language. After this follow tables giving the mean solar times of transit for each day of the Sun and of about twenty-five stars, besides ephemerides of the planets. These tables have been carefully prepared from the 'Nautical Almanac,' and are quite trustworthy. An appendix gives descriptions and prices of the various forms of small Transit Instruments which Mr. Clark has constructed, with testimonials to their efficiency. Colonel Tupman, Mr. Lecky, and other Fellows of the Royal Astronomical Society speak in high terms of their stability and general performance; and it would appear that Mr. Clark's endeavour to popularize transit observation has already met with a considerable and well-deserved amount of appreciation and success.

* Astr. Nach. No. 2568.

† Transit Tables for 1884. Giving the Greenwich Mean Time of the Transit of the Sun, and of certain Stars, for every Day in the Year, with an Ephemeris of the Sun, Moon, and Planets. Computed from the 'Nautical Almanac' for Popular Use. By Latimer Clark, M.I.C.E., &c.—London and New York: E. and F. N. Spon. 1884.

Remarkable Flight of Telescopic Meteors*.—Prof. W. R. Brooks, who rediscovered Comet Pons last September, describes a remarkable observation, which he was fortunate enough to make whilst engaged in comet-seeking, in the following terms :—" Whilst sweeping on the evening of November 28th, it was my pleasure to observe a wonderful shower or flight of telescopic meteors, about ten degrees above the horizon and near the sunset-point. They were very small, none of them visible to the naked eye, most of them leaving a faint train, visible in the telescope for one or two seconds. The motion of most of them was to the northward, with an occasional group to the south of the Sun, moving southward. This observation occurring at the time when the unusual



Flight of Telescopic Meteors.
(Erect view.)

red-light phenomenon was at its height, the theory is suggested of a possible connection between that phenomenon and the passage of the Earth through a mass of meteoric matter more or less attenuated." Referring to the drawing given above, he adds, " As may readily be inferred, the wonderful sight is a difficult one to represent in a drawing, but I have endeavoured to give some idea of the appearance at its maximum stage. The instrument used was my nine-inch reflector with comet eyepiece giving a field of one and one-half degrees. The field shown in the drawing was a few degrees north of the sunset-point and about ten degrees above the horizon. The faithful comet-seeker frequently in a single night's work encounters numerous telescopic meteors singly, very rarely two at once; but this flight is quite unprecedented in my experience."

* Sidereal Messenger, Vol. ii. No. 10.

Mr. Barnard, well known as another industrious and successful comet-seeker, has noted similar phenomena during the period in which the remarkable red sunsets were most conspicuous. One of his observations is thus described in the 'Sidereal Messenger':—"So late as Dec. 15, he saw with the telescope small bright bodies close to the Sun. They were visible at the rate of five or six per minute, and were all moving to the north of east quite rapidly. Occasionally a larger body was seen to flash across the field, blurred by being out of focus. Generally they looked like little stars, many as bright as those of the first magnitude. Mr. Barnard could follow the slower-moving ones with the telescope for five or six degrees from the Sun, where they became faint and were lost. He was unable to detect any crossing the Sun; they seemed to be some distance from it, and required generally an increase of focal distance to be seen favourably. He thinks they are small particles drifting with the air-currents at considerable altitude."

Unusual reversal of Lines in the Summit of a Solar Prominence *.—On Oct. 17, 1883, Prof. Young observed an unusual phenomenon in a prominence connected with the large and active group of spots which at that time was just passing off the Sun's disk. The prominence had the very common form of a number of overlapping arches, with a sort of cap above them. Its elevation was about 2′, and its extent along the circumference was a little less. The peculiar features were the extreme brilliance of the cloud-cap at the summit of the prominence, and the perfect delineation of the form of this cloud in certain spectrum-lines which ordinarily are reversed only at the base of the chromosphere, while at the same time certain other lines, which not unfrequently are reversed at considerable elevations, showed its form only very faintly or not at all. The brightness of the cloud-cap as seen on the C line was simply dazzling, brighter, Prof. Young states, than any prominence or any part of one he ever remembers to have seen. At the same time λ 6676·9 and λ 7055 showed the top of the cloud as well and brightly as C does under usual circumstances. The chromosphere was faintly visible on the same lines, but the stems and lower portion of the cloud were not seen at all upon them. A considerable number of lines were reversed in the chromosphere at the base of the spectrum without being traceable in the cloud-cap; amongst these was the coronal line λ 5315·9. D_3, F, Hγ (λ 4340), and *h* were conspicuous, showing the entire prominence, the cloud-cap being very bright; but H and K showed only the cloud-cap, the stems of the prominence not being visible upon them, although both lines were well reversed, not only in the chromosphere, but also on the face of the Sun over all the facular region surrounding the spots. The sodium lines D_1 and D_2 showed the cloud-cap faintly, but unmistakably; the magnesium lines b_1, b_2, b_4 also showed it faintly; b_3 was a little more conspicuous. But seven other lines were seen as distinctly reversed in the cloud-cap as the two lines below C had been: these were λ 5017·6 and

* 'Science,' Vol. ii. No. 40.

4923·1 (both attributed to iron), Lorenzoni's *f* (λ 4471·2), and four lines between this latter and F. There was no considerable motion-displacement exhibited by any of the lines, nor did the shape of the prominence change much during the time of observation from 8^h 45^m to 9^h 30^m G.M.T. Prof. Young considers that this cloud may perhaps have been identical with a remarkably bright facular *bridge* which was observed two days earlier spanning the largest of the spots which composed the group.

The Brussels Observatory.—We hear with regret that ill-health has caused M. Houzeau to retire from his position of Director of the Brussels Observatory. Appointed to the Directorship of the Observatory in March 1876, M. Houzeau has thoroughly reorganized all departments; under his direction two equatoreals (6 and 9 inch) and a Meridian circle by Repsold have been erected, a spectroscopic department has been formed, and in the meteorological department a complete system of self-recording instruments has been added, and the instrumental readings are published in a daily bulletin, whilst the high value of the numerous bibliographic works which he has executed in conjunction with M. Lancaster, librarian of the Observatory, and the great labour involved in their preparation are recognized by astronomers of all nations. To the energy of M. Houzeau the participation of Belgium in the observation of the Transit of Venus is mainly attributable, and also the erection of a new observatory at Brussels. Herr N. de Konkoly is mentioned as his probable successor.

Hongkong Observatory.—We announced in the 'Observatory' for last November that it was intended that Dr. Doberck, the director of the new observatory at Hongkong, should commence his official duties by making a tour of inspection to the various observatories on the Chinese coast and on the islands of the eastern seas, in order to examine their instruments and records, and, if possible, to arrange that synchronous observations should be taken and regular reports sent to the Hongkong Observatory, so that weather forecasts might be prepared. Dr. Doberck returned from this mission on Nov. 5. With a view to promoting uniformity in observing, he has prepared a little manual of instructions for making meteorological observations for the use of observers at the various stations in the treaty-ports, and for commanders of vessels navigating the China seas. The 'Hongkong Free Press,' remarking on the return of Dr. Doberck, says:—"It is of the first importance, in order to be able to arrive at any definite results, so as to be able to forecast the weather, and compile a reliable weather table, that the meteorological observations conducted at different ports on the China coast should be made at the same hour, in the same manner, and by instruments corrected to the same standard. It is to be hoped, therefore, that Sir Robert Hart will allow his able staff to take part in this work, and

Approximate Times of Transit of the great Red Spot and Equatoreal White Spot across the Central Meridian of Jupiter in February 1884.

Day of Month.	Red Spot on C.M.	White Spot on C.M.	Day of Month.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	10·3	8·3	16.	7·6	12·2
2.	6·1	13·8	17.	13·4	7·9
3.	11·9	9·5	18.	9·3	13·3
4.	7·8	15·0	19.	5·1	8·9
5.	13·5	10·6	20.	10·9	14·5
6.	9·4	6·3	21.	6·7	10·2
7.	15·2	11·8	22.	12·5	5·9
8.	11·0	7·5	23.	8·4	11·4
9.	6·9	13·0	24.	14·1	7·1
10.	12·7	8·6	25.	10·0	12·5
11.	8·5	14·2	26.	5·8	8·2
12.	14·3	9·8	27.	11·6	13·7
13.	10·1	5·5	28.	7·5	9·4
14.	6·0	11·0	29.	13·3	14·9
15.	11·8	6·7	Mar. 1.	9·2	10·6

The observations of the central transits of the red spot obtained here subsequently to those given in the last number are:—

		On C.M.	Long. of Spot.
1883, December	20......	$19^h\ 50^m$	$75^{\circ}\!\cdot\!9$
1884, January	8......	10 35	80 ·9
"	10......	12 14	81 ·9
"	11......	17 56	79 ·2

On Jan. 8 the spot was very carefully observed under good definition. Its form seems unaltered, but the southern side is apparently the darkest. I believe that the faintness of this object is now increasing. The middle of the spot very slightly follows the middle of the hollow in the S. equatoreal belt, which continues to be very conspicuous.

I saw the equatoreal white spot as it came to the C.M. on January 11, $10^h\ 46^m$, when the longitude, computed on the daily rate of $878^{\circ}\!\cdot\!46$, was $77^{\circ}\!\cdot\!6$. It was by no means a prominent feature at that time; but its visible aspect varies considerably and in an irregular manner, so that it may since have regained its former brightness.

Mr. N. E. Green observed the white spot central on Jan. 24, $13^h\ 30^m$. Its position was towards the preceding end of the red spot. I find the longitude from this observation to have been $77^{\circ}\!\cdot\!7$, which is nearly identical with that derived from the transit observed at Bristol on Jan. 11, as above.

The red spot now offers an excellent means of testing the efficiency of telescopes as regards the definition of planetary markings. It must be considered a creditable performance for an instrument to show the outline of the spot steadily and well.

Bristol, 1884, Jan. 26. W. F. Denning.

Elements and Ephemeris of Comet 1883 *b* (*Pons-Brooks*).
By MM. Schulhof and Bossert *.

Collecting all the published observations of the comet up to 1884 Jan. 9, MM. Schulhof and Bossert have formed the following five normal places reduced to the mean equinox of 1880·0:—

Berlin M.T.	α app.	δ app.
1883 Sept. 8·5....	247° 39′ 39″·33	+63° 50′ 0″·09
Sept. 29·5....	246 39 13 ·27	59 14 9 ·81
Oct. 27·5....	253 54 29 ·81	53 30 33 ·70
Nov. 22·5....	270 8 26 ·92	48 38 5 ·61
Dec. 11·5....	291 24 56 ·13	42 42 58 ·85

From these observations the following corrected elements have been computed:—

T	1884, Jan. 25·754599, Berlin Mean Time.	
ω	199° 11′ 33″·21	Eq. and eclip. 1880·0.
☊	254 5 42 ·29	
i	74 2 35 ·72	
e	0·9549960	
log *q*	9·8897099	

These elements give the following errors, O – C, for the normal places:—

	cos δ*d*α.	*d*δ.
1883 Sept. 8·5......	−1″·58	+1″·64
Sept. 29·5......	+1 ·00	+0 ·27
Oct. 27·5......	+2 ·19	+0 ·64
Nov. 22·5......	−0 ·08	−0 ·70
Dec. 11·5......	+0 ·14	−0 ·74

Ephemeris for Berlin Midnight.

1884.	R.A. h m s	Dec. ° ′ ″	Log Δ.	Brightness.
Jan. 31	0 30 39·80	−27 0 58·3	9·91849	77·58
Feb. 2	0 37 12·12	29 10 56·3	9·93294	
4	0 43 18·60	31 11 26·6	9·94698	65·60
6	0 49 1·66	33 3 13·6	9·96083	
8	0 54 23·75	34 47 4·4	9·97408	55·03
10	0 59 27·28	36 23 36·4	9·98678	
12	1 4 15·03	37 53 35·1	9·99894	46·00
14	1 8 48·90	39 17 37·3	0·01051	
16	1 13 11·26	40 36 18·4	0·02149	38·48
18	1 17 24·21	41 50 11·0	0·03188	
20	1 21 29·73	42 59 45·4	0·04170	32·30
22	1 25 29·60	44 5 29·4	0·05096	
24	1 29 25·55	45 7 48·7	0·05966	27·28
26	1 33 19·16	46 7 6·5	0·06785	
28	1 37 11·85	−47 3 44·3	0·07553	23·19

The brightness at the time of discovery is taken as unity.

* Astron. Nachr. No. 2569.

Elements of the Minor Planet 235, *Carolina.*
(Communicated by Rear-Admiral SHUFELDT.)

THE following elements of planet 235, discovered by Herr Palisa at Vienna on 1883, Nov. 28, and which has since received the name of Carolina, have been computed by Professor E. Frisby from the following observations made at the U. S. Naval Observatory, Washington, by Commander W. T. Sampson.

1883.	Wash. M. T. h m s	App. α. h m s	App. δ. ° ′ ″
Dec. 1	13 13 19·5	3 16 31·80	+15 51 0·8
6	11 59 7·0	3 12 39·75	+15 50 6·1
21	12 12 46·0	3 3 48·88	+15 57 11·9

From which are derived the following elements:—

Epoch.............. 1883, Dec. 21·5, G.M.T.

	° ′ ″	
M	275 3 21·2	Mean Equinox 1883·0.
☊	67 13 21·2	
π − ☊	86 50 22·8	
i	8 34 28·1	
φ	4 10 44·2	
log a	0·471817	
μ	695·471	

Comparison with the middle place gives

$$\delta\lambda \cos\beta = 0''\cdot0 \qquad \delta\beta = 0''\cdot0.$$

$$\left.\begin{aligned} x &= [9\cdot995856]\, r \sin(v + 243^\circ\ 49'\ 53''\cdot8) \\ y &= [9\cdot951720]\, r \sin(v + 157\ \ 48\ \ 10\ \cdot8) \\ z &= [9\cdot669480]\, r \sin(v + 138\ \ 36\ \ \ 6\ \cdot8) \end{aligned}\right\} 1884\cdot0.$$

Differences of Right Ascension and Declination between Iapetus and the centre of Saturn. By A. MARTH*.

	α−A. s	δ−D. ″		α−A. s	δ−D. ″
Feb. 2	−37·4	−143	Feb. 16	−10·6	−186
3	−36·8	−154	17	− 7·6	−179
4	−36·0	−164	18	− 4·7	−172
5	−34·9	−173	19	− 1·7	−163
6	−33·6	−181	20	+ 1·3	−153
7	−32·1	−187	21	+ 4·3	−142
8	−30·3	−192	22	+ 7·2	−131
9	−28·4	−196	23	+10·1	−119
10	−26·2	−199	24	+12·9	−106
11	−24·0	−200	25	+15·6	− 92
12	−21·5	−200	26	+18·2	− 78
13	−18·9	−198	27	+20·6	− 63
14	−16·2	−195	28	+23·0	− 49
15	−13·4	−191	29	+25·2	− 34

* Monthly Notices, Vol. xliii. No. 8.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

Mimas (East).		h
Feb.	2	7·5
	15	12·2
	16	10·8
	17	9·4
	18	8·0

Mimas (West).		h
Feb.	6	13·4
	7	12·0
	8	10·7
	9	9·3
	23	12·5
	24	11·2
	25	9·8
	26	8·4
	27	7·0

Hyperion.		h
Feb.	4	3·8
	25	9·4

Enceladus.		h
Feb.	1	20·7
	3	5·6
	4	14·5
	5	23·4
	7	8·3
	8	17·1
	10	2·0
	11	10·9
	12	19·8
	14	4·7
	15	13·6
	16	22·5
	18	7·4
	19	16·2
	21	1·1
	22	10·0
	23	18·9
	25	3·8
	26	12·7
	27	21·6
	29	6·5

Tethys.		h
Feb.	1	7·9
	3	5·2
	5	2·5
	6	23·8
	8	21·1
	10	18·4
	12	15·7
	14	13·0
	16	10·4
	18	7·8
	20	5·1
	22	2·4
	23	23·5
	25	21·0
	27	18·3
	29	15·7

Titan.		h
Feb.	3	11·2
	19	9·2

Dione.		h
Feb.	2	7·3
	5	1·0
	7	18·7
	10	12·3
	13	6·1
	15	23·8
	18	17·5
	21	11·1
	24	4·8
	26	22·3
	29	16·0

Rhea.		h
Feb.	4	4·7
	8	17·1
	13	5·6
	17	18·0
	22	6·5
	26	19·0

The following table will furnish the means for inferring approximate distances and position-angles of the satellites from the diagram given in Plate I., opposite page 26.

	Position-angle of Minor Axis of Ring.		Semi-major axis of orbit of Titan.
Jan. 29	356° 43′	Jan. 29	195″·8
Feb. 8	356 43	Feb. 8	192 ·3
18	356 42	18	188 ·8
28	356 39	28	185 ·3

Astronomical Memoranda, 1884, *February.*

Sun. Feb. 1, sets 4^h 46^m, rises 19^h 40^m; Feb. 11, sets 5^h 4^m, rises 19^h 23^m; Feb. 21, sets 5^h 23^m, rises 19^h 3^m; Feb. 29, sets 5^h 37^m, rises 18^h 46^m.

Equation of Time:—Sun *after* Clock, Feb. 1, 13^m 47^s; Feb. 11, 14^m 28^s; Feb. 21, 13^m 52^s; Feb. 29, 12^m 37^s.

Sidereal Time at Mean Noon:—Feb. 1, $20^h\ 44^m{\cdot}4$; Feb. 11, $21^h\ 23^m{\cdot}9$; Feb. 21, $22^h\ 3^m{\cdot}3$; Feb. 29, $22^h\ 34^m{\cdot}8$.

Moon.	*sets.* h	m		*rises.* h	m		*rises.* h	m
Feb. 1..	10	25	Feb. 11..	5	58	Feb. 21..	16	4
2..	11	41	12..	7	8	22..	16	45
3..	12	55	13..	8	15	23..	17	21
4..	14	8	14..	9	23	24..	17	54
5..	15	16	15..	10	27	25..	18	22
6..	16	17	16..	11	31	26..	18	50
7..	17	9	17..	12	32	27..	*sets.*	
8..	17	53	18..	13	31	28..	8	9
9..	18	28	19..	14	27	29..	9	26
10..	19	0	20..	15	17			

First Quarter, Feb. 3, $17^h\ 57^m$; Full Moon, Feb. 10, $16^h\ 48^m$; Last Quarter, Feb. 18, $15^h\ 13^m$; New Moon, Feb. 26, $6^h\ 35^m$.

Mercury is a morning star throughout the month. He is at greatest elongation, 26° 12′ W., Feb. $13^d\ 17^h$.

Venus is an evening star. Diameter:—Feb. 1, 12″·4; Feb. 29, 14″·1. Illuminated portion of disk, Feb. 14, 0·814.

Feb. 1,	R.A. $23^h\ 3^m{\cdot}3$,	Dec. 7° 29′ S.,	tr. $2^h\ 19^m$,	sets $7^h\ 44^m$
29,	1 6 ·4,	7 3 N.,	2 32	9 11

Mars is retrograding in Cancer. Diameter:—Feb. 1, 16″·6; Feb. 29, 14″·5. Illuminated portion of disk 0·991 on Feb. 14.

Feb. 1,	R.A. $9^h\ 3^m{\cdot}6$,	Dec. 21° 32′ N.,	tr. $12^h\ 16^m$,	sets $20^h\ 22^m$
29,	8 27 ·0,	23 19 N.,	9 50	18 5

Jupiter is retrograding between Cancer and Gemini. Diameter: —Feb. 1, 42″·7; Feb. 29, 40″·5.

Feb. 1,	R.A. $8^h\ 0^m{\cdot}4$,	Dec. 21° 10′ N.,	tr. $11^h\ 14^m$,	sets $19^h\ 17^m$
29,	7 48 ·8,	21 43 N.,	9 12	17 22

Saturn is stationary, Feb. $3^d\ 14^h$, and in quadrature with the Sun, Feb. $22^d\ 5^h$.

Feb. 1,	R.A. $4^h\ 6^m{\cdot}1$,	Dec. 19° 3′ N.,	tr. $7^h\ 20^m$,	sets $15^h\ 7^m$
29,	4 8 ·6,	19 17 N.,	5 33	13 22

	Outer Ring. Maj. Axis.	Min. Axis.	Inner Ring. Maj. Axis.	Min. Axis.	Ball. Diam.
Feb. 3	43″·15	18″·50	28″·69	12″·30	17″·2
23	41 ·58	17 ·92	27 ·65	11 ·91	16 ·6

The south side of the rings is visible, the elevation of the Earth above their plane being 25° 23′ on Feb. 3, and 25° 31′ on Feb. 23; and of the Sun 26° 6′ and 26° 11′.

Uranus is near β Virginis.

Feb. 1,	R.A. $11^h\ 53^m{\cdot}0$,	Dec. 1° 37′ N.,	tr. $15^h\ 6^m$,	rises $8^h\ 53^m$
29,	11 49 ·5,	2 1	13 12	7 5

Neptune is in quadrature with the Sun Feb. 7^d 2^h.

	R.A.	Dec.	tr.	sets
Feb. 1,	3^h $5^m.3$,	15° 35′ N.,	6^h 20^m,	13^h 49^m
29,	3 6 ·4,	15 42 N.,	4 31	12 1

Phenomena.

	G. M. T. h	m	
Feb. 1	5	40	J. i. Oc. D.
	8	16	J. i. Ec. R.
	15	6	J. ii. Tr. I.
	18	1	J. ii. Tr. E.
2	10	58	J. iii. Oc. D.
	15	52	J. iii. Ec. R.
3	10	10	J. ii. Oc. D.
	13	46	J. ii. Ec. R.
4	11	48	B.A.C. 1119 Oc. D. 130°.
5	4	20	δ^2 Tauri Oc. D. 32°.
	4	56	δ^3 Tauri Oc. D. 137°.
	7	8	J. ii. Tr. E.
	15	51	J. i. Tr. I.
6	8	16	119 Tauri Oc. D. 96°.
	9	0	120 Tauri Oc. D. 85°.
	12	58	J. i. Oc. D.
	15	42	J. i. Ec. R.
7	10	17	J. i. Tr. I.
	12	37	J. i. Tr. E.
	13	40	26 Geminorum Oc. D. 103°.
8	7	24	J. i. Oc. D.
	10	10	68 Geminorum Oc. D. 24°.
	10	10	J. i. Ec. R.
	17	22	J. ii. Tr. I.
9	7	3	J. i. Tr. E.
	12	11	B.A.C. 2872 Oc. D. 145°.
	14	17	J. iii. Oc. D.
10	12	26	J. ii. Oc. D.
	16	22	J. ii. Ec. R.
11	6	53	16 Sextantis Oc. R. 322°.
12	6	31	J. ii. Tr. I.
Feb. 12	9	25	J. ii. Tr. E.
13	7	35	J. iii. Tr. E.
	14	43	J. i. Oc. D.
14	12	2	J. i. Tr. I.
	14	22	J. i. Tr. E.
15	9	9	J. i. Oc. D.
	12	5	J. i. Ec. R.
16	6	29	J. i. Tr. I.
	8	49	J. i. Tr. E.
	9	20	J. iv. Oc. D.
	12	15	λ Virginis Oc. R. 220°.
	13	48	J. iv. Oc. R.
	15	45	J. iv. Ec. D.
17	6	34	J. i. Ec. R.
	14	44	J. ii. Oc. D.
19	8	49	J. ii. Tr. I.
	11	43	J. ii. Tr. E.
	17	40	B.A.C. 5700 Oc. D. 104°.
20	7	25	J. iii. Tr. I.
	10	59	J. iii. Tr. E.
	16	29	J. i. Oc. D.
21	8	16	J. ii. Ec. R.
	13	48	J. i. Tr. I.
	16	8	J. i. Tr. E.
22	10	56	J. i. Oc. D.
	14	0	J. i. Ec. R.
23	8	15	J. i. Tr. I.
	10	35	J. i. Tr. E.
24	8	29	J. i. Ec. R.
	16	5	J. iv. Tr. I.
26	11	10	J. ii. Tr. I.
	14	4	J. ii. Tr. E.
27	10	52	J. iii. Tr. I.
	14	26	J. iii. Tr. E.
28	10	51	J. ii. Ec. R.
	15	36	J. i. Tr. I.
29	12	43	J. i. Oc. D.
	15	55	J. i. Ec. R.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

Publications received:—Dr. F. Terby, *Remarques à propos des recentes observations de M. Schiaparelli sur la planète Mars* (Monthly Notices Royal Astronomical Society); *Sur l'existence et sur la cause d'une périodicite mensuelle des aurores boréales, Note sur l'aurore boréale du* 2 Oct. 1882, *ditto* 17 Nov. 1882, *Aspect de la grande comète* (b) 1882, 1^er^, 2^e^, 3^e^ *notices*, and *Observation de la lumière zodiacale et d'un petit bolide* (Bulletins de l'Académie Royale de Belgique).

ERRATA.

No. 75, page 214, line 27, *for* edge of this ball *read* edge of this belt.
78, 306, 47, *for* 15′ 1″·78 *read* 15′ 31″·78.
81, 3, 44, *for* by 360°. Whilst *read* by 360° whilst.
81, 27, 33, *for* $21^h\ 19^m$, *read* $19^h\ 21^m$.

The Rev. Stephen H. Saxby also desires the following correction to be made in his letter, p. 19:—

No. 81, page 19, line 12, *for* easily divides, *read* on one occasion (1883, Nov. 16) showed Encke's division in the eastern portion of.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 83. MARCH 1. 1884.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1884, February 8.

E. J. Stone, M.A., F.R.S., *President*, in the Chair.

Secretaries: J. W. L. Glaisher, M.A., F.R.S., and E. B. Knobel.

The Minutes of the last Anniversary Meeting were read and confirmed.

The report of the Auditors for the past year was read by *Mr. Lecky*, and was to the effect that having examined the Treasurer's account, and the account of the assets and property of the Society, they certified them to be correct; that the cash in hand on the 31st of December 1883 was £309 8*s*. 7*d*.; with regard to the funded property of the Society, that £500 Metropolitan Board of Works Stock had been purchased; that the Society's rooms had been decorated and repaired at a cost of £529 18*s*. 11*d*.; that the books and instruments and other effects had been examined as far as possible, and found to be in a satisfactory condition; and that they had laid upon the table a list of the names of those Fellows who were in arrear for sums due at the last General Meeting.

Mr. Glaisher gave a summary of the first part of the report of the Council for the past year. During the year 30 ordinary Fellows had been elected and 16 had died, 9 had resigned, and one had been expelled, the total number of Fellows now being 602, an increase of 4. Four new Associates had been elected and one had died, the total number of Associates now being 49.

The following are the names of the Fellows who have died during the year:—Francis Abbott, George Bishop, C. J. Corbett, W. A. Cross, J. H. Dallmeyer, Joseph Drew, Rev. N. S. Godfrey, F. C. Green, Gen. Sir H. Harness, Julius Page, Gen. Sir E. Sabine, Rev. A. Smith, Prof. H. J. S. Smith, William Spottiswoode, Thos. Warner, and T. R. White; the Associate was M. Yvon Villarceau.

The usual reports from a large number of observatories had been received and were briefly summarized. The notes on the progress of astronomy during the year were numerous and important, the subjects referred to being the following:—Transits of Mercury, 1677–1881. —Harmonic Analysis of Tidal Observations.—Longitude Determinations.—Schiaparelli's Observations of Mars.—Auwers' Mittlere Oerter von 83 Südlichen Sternen.—M. Magnus Nyrén's Investigation of the Constant of Aberration.—Discovery of Minor Planets. —Dr. Meyer's Observations of Saturn.—The recent Transit of Venus.—Mr. Hill's paper "On certain possible Abbreviations in the Computation of the Long-period Irregularities of the Moon's Motion due to the direct Action of the Planets."—Prof. H. M. Paul's determination of the Semi-diameter of the Moon.—Photographing the Corona without an Eclipse.—Comets of 1883.— The Unification of Longitudes and Time.—The Figure of the Earth. —The Total Solar Eclipse of 1883, May 6.—Potsdam Physical Observatory.

The first-mentioned note was a notice of Prof. Newcomb's discussion of the observed Transits of Mercury, in which he treats of the questions:—"Do the Transits of Mercury prove or disprove the hypothesis of the variability of the Earth's axial rotation?" and, secondly, "What are the possible causes of the excess of motion of the perihelion of Mercury?" his conclusion being that "inequalities in the motion of the Moon not accounted for by the theory of gravitation really exist, and exist in such a way that the mean motion of the Moon between 1800 and 1875 was really less than it was between 1720 and 1800." He also finds "that the observed centennial motion of the perihelion of Mercury is greater by 43″ than the theoretical motion, computed from the best attainable masses of the planet." In seeking for a possible explanation of this excess of motion, he concludes that "assuming that we are still to look to a more exact determination of the astronomical character of the phenomena for a solution of the question, the necessary steps are an exact determination of the mass of Venus from the periodic perturbations of the inner planets; an investigation of the secular motions of the planes of the orbits of those planets; and the comparison of the theoretical and observed secular motion of the perihelion of Venus." On the importance of Transits of Mercury, Prof. Newcomb says:—"Until the question of possible changes in the Earth's axial rotation shall be placed on a firm basis, or until the theory of the Moon's mean motion shall be so perfected that these changes can be determined with precision from observations of that satellite, transits of Mercury must be regarded with the greatest interest, as affording independent determinations of the variations in question. The November Transits will long be most favourable for this purpose, for the reason that the series of observed November Transits extends back nearly a century before the first well-observed May Transit. It is to them, therefore, that we must principally look for light upon this question."

The note respecting Dr. Huggins's experiments in photographing the solar corona stated that during the past year Dr. Huggins had continued his experiments with a more suitable form of apparatus, the Misses Lassell having kindly placed at his disposal a 7-foot Newtonian telescope, constructed by the late Mr. Lassell. The use of cells containing coloured fluids had been discontinued as the rapid decomposition of the fluid under the Sun's light and the formation of a fine deposit on the surface of the plates might give rise to misleading appearances. Between April 2 and September 4, about 50 photographs were taken, all of which show a more or less coronal appearance. From an inspection of the negatives, Messrs. Lawrance and Woods, who observed the eclipse of 1883, May 6, at Caroline Island, consider that "Dr. Huggins's coronas are certainly genuine as far as 8′ from the limb." A careful examination of the photographs taken on April 3, May 31, and June 6 shows a rift similar to that which is a conspicuous feature in the photographs taken during the eclipse. A committee of the R. S. has been appointed to arrange for taking photographs of the Sun by Dr. Huggins's method at some place of high elevation.

The paper on the Transit of Venus stated that 34 observations of contact were made at Ingress and 39 at Egress; that the whole of the time determinations are satisfactory and the local time of each observation is computed, and the reductions of longitudes are in an advanced state. The note on the unification of longitudes and time stated that all the delegates to the Geodetic Congress at Rome, with the exception of the French, voted for the adoption of the meridian of Greenwich in reckoning longitudes and international time. The French delegates proposed an initial meridian 18° W. of Greenwich, through the island of Ferro, for longitudes, M. Faye agreeing to adopt the meridian of Greenwich for international time. The Railway Companies of the United States and Canada have been the first to take practical steps towards the unification of time, by adopting throughout their system of rails (100,000 miles in length) standard time reckoned from meridians respectively 4^h, 5^h, 6^h, and 7^h W., the minutes and seconds being identical with G.M.T. The new "Eastern time," 5^h slow on G.M.T., was introduced on November 18 last by the transmission of signals from Yale College Observatory.

The President in announcing the award of the Gold Medal to Mr. Common, addressed the Society as follows:—

GENTLEMEN,—Your Council have awarded the Gold Medal to Mr. A. A. Common, for his Photographs of Celestial Bodies.

In this award the Council have been less influenced by originality in the methods adopted than by the great practical success which has attended Mr. Common's efforts in a most important and interesting field of astronomical research. It appears that in 1874 Mr. Common was in possession of a five-and-a-half-inch refractor, and was even then engaged in astronomical photography. He soon, however, found it desirable to improve his instrumental means; and provided himself with two disks of glass of seventeen inches diameter, with the intention of grinding his own mirrors and mounting a reflector of this size. But fortunately, perhaps, for our science, Mr. Common abandoned this idea, and supplied himself with an eighteen-inch mirror by Calver. The mounting for this mirror was designed by himself, and executed under his direct personal superintendence, and he was able to commence work with this powerful instrument in 1877. But even this instrument appeared insufficient to meet Mr. Common's requirements, and he soon gave an order to Mr. Calver for a mirror of three feet diameter. The first disk of this size burst into fragments under the tool, but a second disk was successfully worked and the mirror mounted in July 1879. A description of this instrument is given in the 'Memoirs,' Vol. xlvi. The mounting shows in every direction great engineering skill, guided by the experience, gained in the use of the smaller instruments, of the actual requirements for successful astronomical work. The method of relieving the weight by floating parts of the mounting in mercury, and the special adjustments for the use of the instrument for photographic purposes, by allowing of an independent motion of the axis, are deserving of attention. This powerful instrument has been used in the observation of the fainter satellites of Saturn and of Mars, but has been more especially devoted to photographic work. The first attempt at a photograph of the Nebula of Orion was made on 1880, Jan. 20. The result was a failure. The stars were seen as lines, and the nebula proper presented merely a faint stain upon the plate. But such failures only suggested the necessity of improved clock-driving and the use of more sensitive plates. Thus availing himself of every increase of the sensitiveness of the prepared photographic plates, and continually improving the control of the driving-clock, Mr. Common was able on 1881, Jan. 24, to obtain a photograph of Comet *b*, which is probably the earliest successful photograph of any comet; whilst on 1882, March 17, a photograph of the Nebula of Orion was obtained, which was exhibited at the May meeting of the Society, and which excited the admiration of all the astronomers who had an opportunity of inspecting it.

But still the clockwork admitted of some further improvements. It was possible to place a wire to afford a visible point of reference of the fixed direction of the instrument relatively to the stars, and thus to allow of a great extension of the time of exposure of the plates without destroying definition.

These practical improvements rendered it possible to secure the splendid photograph of the nebula which was presented to the

Society in March 1883. This photograph was taken on 1883, Jan. 30.

The work has been continued, and to allow for the effects of different exposures and the sensitiveness of the plates to light of different degrees of refrangibility a long series of experiments have been carried out with exposures increasing from 1^m to 90^m. It appears that even with the very long exposures more details are brought out by every increase in the length of exposure, and that the extreme limit of useful exposure has not even been reached at $1^h\ 30^m$. The success of these long exposures with this powerful instrument has opened out a new field of research, by which the accumulating effect of the light of faint stars, too faint even for observation by the eye, have been registered upon the photographic plate. It is, indeed, difficult to over-estimate the interest with which the results which Mr. Common may obtain in this direction will be watched by astronomers. In addition to the work mentioned, Mr. Common has obtained some beautiful photographs of the planets Jupiter and Saturn; and has recently applied himself with success to obtaining photographic star-maps to stars of the 11th magnitude, a field in which Professor Pickering has already made some progress.

Such, Gentlemen, is a bare record of the steps which have led Mr. Common to success, and the justification of the Council in awarding to him the Medal of the Society. The lesson taught is not a new one. The records of our Society are rich in the labours of our amateur astronomers. The amateur who can provide himself with sufficient instrumental means for original research need fear no professional rivalry; untrammelled by the necessity of continuing observations whose value largely depends on their continuity, having the power of taking up any subject he pleases, pursuing it so long as he believes in the possibility of success, without fear or responsibility of charges of wasted time and wasted means, he possesses advantages which are priceless in the tentative and experimental stages of any work.

It is in work of this class that the most striking advantages in our science must be expected; and such work will most certainly repay, by the gratification of personal success, the efforts of those who devote themselves to original work in our science, and the field of research presented is absolutely boundless.

Addressing *Mr. Common* the President continued:—

Mr. Common,—It is with the greatest pleasure I place in your hands this Medal, as a recognition, on the part of the Royal Astronomical Society, of the great value of your contributions to the advancement of our science.

Mr. De La Rue. Mr. President and fellow members. I have had a resolution placed in my hands, which I feel great pleasure in moving, and which really requires no words of mine to recommend to your notice. We have had placed before us, through the labours of our Secretaries, a most able report, and this year the report has contained an unusual amount of matter of great interest. (Applause.) We have also had a most able and discriminating address from the President, telling us the circumstances under which the award of the gold medal has been made to Mr. Common. The President has said truly that there are branches of astronomy better carried on by amateurs, unfettered in their efforts by public bodies, than by professional astronomers. He has shown very clearly how Mr. Common has progressively overcome difficulty after difficulty, and succeeded, beyond all that could have been possibly expected a few years ago, in photographing astronomical objects. Such results could scarcely have been anticipated, and will be found very difficult to surpass. (Applause.) We have had before us also the Auditors' Report and our late Treasurer's account; and I cannot pass over this without saying a word or two in praise of the devotion of our late Treasurer to the interests of the Society. (Applause.) On his resigning the post of Treasurer some time ago, he spoke to me of the interest he felt in the affairs of the Society, and up to the day of his leaving the office he devoted himself most zealously to the interests of the Society; I therefore beg to move that the report now read be received and adopted, and that it be printed for circulation in the usual manner, together with the report of the Auditors, and the President's—I must put in one more word—excellent address.

Dr. Gill. Mr. President and Gentlemen: I have been requested to second the motion which has just been so ably moved by Mr. De La Rue. I have myself been an amateur astronomer, and am now a professional one, and I can most heartily endorse the remarks as to the respective advantages of the two positions which Mr. Stone has so clearly set before us. The Society always well weighs the position of the men and the advantages they have when they make these awards; and I have no hesitation in saying that if the position of an amateur astronomer is considered by anybody to be a disadvantageous one, that at least has not weighed, I am sure, in the feeling of the Council in making this award to Mr. Common. With regard to the gentlemen to whom Mr. De La Rue has so well referred, I have pleasure in seconding that portion of the motion.

Captain Noble. Before the motion is put to the meeting, I think it will be satisfactory to some of the Fellows, and I know it will be to myself, to have some explanation of an item which appeared, I think, in the Auditors' report, viz. that of between £500 and £600 for decorating these rooms. Now, to use a vulgar phrase, it seems to me to be rather a sporting amount. (Laughter.) I remember in my time we have been told we could not have books bought or books bound because we had not money enough; but £500 or £600 for decoration seems a large amount, and it would be satisfactory to me, and probably to other Fellows, to have some explanation of that expenditure.

Mr. Lecky. The Auditors looked into that matter, as it appeared to them also that the amount was heavy; but we must take into consideration that we have these premises rent free, and that this present expenditure of £529 is spread over ten years. (Hear, hear.) We have occupied these rooms ten years, so that really this sum amounts to only about £53 a year for these premises. I do not think I can give any further explanation than that. We all must admire the way in which the work has been done, and the care with which one of our Fellows has watched the progress of the work.

Mr. De La Rue. I think I can appreciate the objection of Captain Noble, which has probably arisen from the insertion of the word "decoration;" it should rather have been "restoration." The decorative part must be a very small item. The charge is really for thoroughly repainting and cleaning the place, and doing whatever there was to be done.

The President then put the resolution to the Meeting, and it was adopted.

Captain Noble. With reference to the question of balloting, might I, Sir, suggest whether it might not be worth while for the new Council to consider the way in which the ballot is taken. I am moved to do this by the fact that I received last night from a gentleman, of whose very existence I was previously ignorant, his proxy for to-day. He imagined he might vote by proxy. Whenever there is an election for the University, I have three or four neighbours who come to me and ask me to attest their proxy; and I think that what is good for a country parson might be good for a country astronomer, who might be very much interested in the result of an election and might wish to vote. The fact is that the present system throws the election into the hands of a few gentlemen who are resident in or near London. There are many Fellows of the Society who take a deep interest in its work, but who do not take an interest to the extent of coming up three or four hundred miles to vote; and if they could have proxy papers properly attested by a magistrate or clergyman on the spot, they might do so. I think it would be worth the while of the Council at all events to consider the question. The existing bye-laws on the subject are express that the voting must be personal; I do not need to be told that; but if you insure the identification of the voter, I do not see why you should drag a man four or five hundred miles to London to give a vote which he might be very glad to give if he were here upon the spot.

The President. You would have to make a formal proposition before the alteration could be made.

Captain Noble. I merely throw out the hint for the consideration of the new Council.

Captain Noble, Mr. Harrison, and Mr. Cushing were then appointed Scrutineers, and the ballot for the election of Officers and

He came to the conclusion that some weight ought to be given to each observation, and gave a formula for combining the weights and given periods together. He thought, however, that if weights were not correctly given, a simple arithmetical mean was the best method.

Nine Members were elected, and Dr. William Huggins, F.R.S., and Rev. S. J. Perry, F.R.S., were elected Associates of the Society.

The Meeting adjourned at $10^h\ 15^m$.

The Red Spot on Jupiter.

On January 27, while observing the transit of the Red Spot across the central meridian of Jupiter, at $11^h\ 10^m$, I noticed that a marked change had occurred since my observations of January 8, 10, and 11, reported in the 'Observatory' for January, p. 54, and 'English Mechanic,' No. 983, p. 448. There was now a very bright region immediately on the south and west borders of the spot, but the following end apparently connected itself with a faint grey belt in nearly the same latitude as the southern side of the spot. I could not, however, be absolutely certain of this connection, for though definition was as nearly perfect as possible, there was a gale of wind blowing, and the telescope shook incessantly. At times, however, I obtained a fairly steady view for a moment, and then the abnormal appearance of the spot struck me very forcibly as one which certainly had not presented itself in any of my previous observations. On the following night, January 28, I re-examined the spot at about $7^h\ 15^m$, when it had just passed the central meridian, and again noticed the distortion of the *f.* end. The S.W. boundary was, as before, remarkably definite, owing to the contrast with the outlying bright zone; but the *s.f.* end of the spot had evidently coalesced with a faint dusky belt lying somewhat to the southward. I wrote to Mr. N. E. Green mentioning the fact, and he has kindly given me some of the results of his observations, which fortunately were made at the same times as my own. He remarks that on January 27 he estimated the red spot central at $11^h\ 15^m$ G. M. T., and had some difficulty in making out the details of the *f.* end. He suspected the connexion with a dark belt on the S. side of the spot; and on the ensuing night, January 28, the idea was fully confirmed. The *f.* end undoubtedly shaded off into the dark belt; in fact, though the S. line of the spot was still definite and rather grey, the *f.* end was not traceable.

On February 6 the spot was further examined by me with special regard to this change of figure. At $9^h\ 29^m$ I judged the spot to have become central, and its appearance, as I saw it with a power of 252 (single lens) on my 10-inch reflector, was as below:—

Definition was unusually sharp. The sketch, which only reproduces a part of the planet's southern hemisphere, includes some extremely curious features, which may be described as follows :—

1. *The great red spot* is seen central and to the S. of the remarkable hollow in the S. side of the great S. belt. The N. border

Red Spot on Jupiter: 1884, February $6^{d}\ 9^{h}\ 29^{m}$.

of the spot is very feeble and indefinite, while its *s. f.* side is seen attached to the belts in higher latitude. Two of these belts are connected by a ligament just where they merge into the spot.

2. *The hollow in the great S. belt* is about central. The shoulders of the belt in which it is formed are very dark; in fact this belt is by far the most conspicuous one visible on the disk.

3. *The narrow though intensely bright division in the great S. belt.*—This feature is not invariably seen, though I believe it is always there. It requires good definition and a steady image; then it may be followed as a bright line of silver separating the belt lengthwise.

4. *The large white spot S. of the p. end of the Red Spot.*—This object, though occupying a similar latitude to the white spot which has been so persistent during the last few years, is a different object, as it is some 87° E.

The fact that the red spot is now actually connected with the grey southern belts of Jupiter is important and suggestive that its material, already much exhausted, will be finally diffused amongst them. If, however, the spot is an opening in the Jovian atmosphere, and the intensely red colour it formerly exhibited (which, it may be remarked, has also been observed in the more prominent belts) the representative hue of the planet's lower atmosphere, then the extinction of the spot will mean the gradual filling up of the cavity by the more highly reflective clouds of the outer strata. Its ultimate combination with one of the longitudinal currents of which the dark belts afford us the visible evidence, will require a different explanation. If this new belt, which now joins

CORRESPONDENCE.

To the Editor of 'The Observatory.'

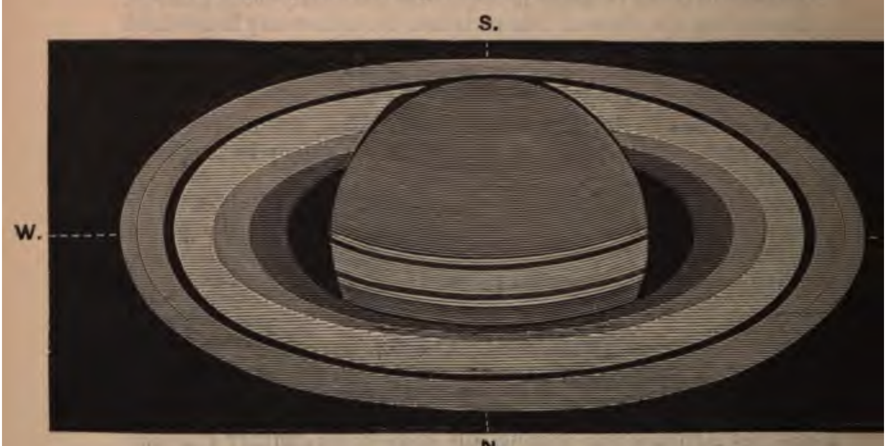
The Narrow Belt on Saturn.

Sir,—

I notice in your Journal for January 1884 a report of the discussion in the Royal Astronomical Society upon a communication of Mr. Ranyard's relative to a narrow belt on *Saturn*. Mr. Ranyard speaks of seeing such a narrow (and dark) belt on the 4th, 13th, and 21st of November, and it also appears to have been seen by Dr. Copeland, November 6.

Mr. Ranyard inquires if this belt has been seen elsewhere. In answer to this inquiry I beg to say that I have seen it at various times since Nov. 8, and that I have drawings of it on Nov. 8, Nov. 24, and Dec. 2. A copy of the last drawing I enclose with this, as it is the best of the three. My notes are as follows:—

Nov. 8. "*The Ball*; the S. hemisphere is dark, the S. pole the darkest. A dark stripe along the S. edge of the bright equatoreal belt. This belt has a dark and narrow stripe along its middle." Nov. 24. Drawing only. Dec. 2 [see the accompanying drawing]. "*The Ball*; the S. pole is mottled, especially so near the shadows. The bright equatoreal belt is bounded on the S. by a narrow dark streak some 2″ wide; it is the darkest thing on the ball. S. of this is an equally narrow bright streak, then S. of this is the nearly



uniform S. hemisphere. N. of the equatoreal bright belt is a narrow dusky belt (1″·5 ?), then a narrow bright belt (1″·5 ?), and then a dark band, which is the dusky ring itself (ring C). The principal division is seen all around, the division in ring A is seen at both ends. The shadow of the ball on the ring is as drawn. It is wider and of a different shape on the preceding side, as drawn. I did not specially look for (nor see) the shadow of the ball on the ring C" [this being a test of good images].

The position of the belt has not greatly changed during the period in which I have seen it. I think it has changed somewhat, however.

I am, my dear Sir,
Very faithfully,
EDWARD S. HOLDEN.

Washburn Observatory,
University of Wisconsin, Madison,
1884, Jan. 21.

SIR,—

The belt on Saturn, which was brought before the notice of the Astronomical Society at their last meeting, I have seen several times; most distinctly on February 2. It was then very distinct, sharply defined on its northern edge, and fading away more gradually to the south, the southern edge being irregular. It appeared to me of a dark grey colour. The equatoreal region of Saturn was very brilliant, and the distinction between it and the northern edge of the belt very marked, more clear than any such difference I have ever seen on the planet during five years' occasional watching. The belt could not be mistaken for a shadow or for the dusky ring, which is quite separate from it. Its curvature is slight. It would be interesting if any one could give the time it first appeared or was first noticed.

Yours faithfully,
EDWARD B. KIRK.

Barrhead, Scotland, 1884, Feb. 5.

The supposed New Star of A.D. 389.

SIR,—

In consequence of a note which I addressed to the 'Athenæum' on this subject (published in the number of that Journal for January 19th), Prof. Steiff of Tübingen has kindly supplied the reference to the passage in Cuspinianus, which I failed to find. It is contained in his 'Commentarii de Consulibus Romanorum,' published at Basle in 1552, twenty-three years after the author's death. Under the consulship of Timasius and Promotus, at p. 518 of that work, he says:—

"Marcellinus sic de his Coss. scribit: Timasii et Promoti consulatu Theodosius Imperator cum Honorio filio suo Romam mense Junio introivit. Congiarium populo Romano tribuit, Urbeque egressus est Calendis Septembris. Per idem tempus grando crepitans per biduum continuum plurima cecidit pecudum arborumque pernicies. Item stella à Septentrione gallicinio surgens, et in modum luciferi ardens, prius quam splendens apparuit, vigesima sexta die desiit esse."

Cuspinianus (or Spieshammer), therefore, as I had concluded, simply quoted the account of the appearance of the star (or rather comet) from Marcellinus. The mistake of supposing he had seen it himself seems to have been made by Cassini, and afterwards by Humboldt, misunderstanding the somewhat ambiguous expression of Tycho Brahe in reference to it. Prof. Steiff notices that it was committed by thinking that Cuspinianus's quotation from Marcellinus terminated at the word "pernicies," and that the last sentence of the above quotation was his own, founded on personal observation. But it could hardly have been fallen into by any one who had referred to Cuspinianus himself, and must rank with those errors which so frequently arise from neglecting to trace a quotation to its original source.

Yours faithfully,

Blackheath, 1884, Feb. 9. W. T. LYNN.

P.S.—It will be remarked (see my letter to you of Nov. 19, printed in January) that Cuspinianus commits two small errors in quoting Marcellinus, giving the word "prius" instead of "potius," and making the visibility of the so-called star last until the twenty-sixth instead of the twenty-third day after its first appearance.

On the Change in the Adopted Unit of Mean Solar Time made in 1864.

SIR,—

Professor Adams's attempts to prove me wrong simply amount to giving a proof of a result, correct on certain assumed conditions, and erroneously applying it to a case where those conditions are violated.

It would be quite possible to represent the motion of the Earth about its axis by $\omega = 360 \times 60 \times 60 + n$, where n is any assigned angle, by adopting a proper unit, T, whose ratio to a sidereal day, S, would be

$$\frac{T}{S} = 1 + \frac{n}{360 \times 60 \times 60},$$

provided $360 \times 60 \times 60$ is also a mere angle; and it would be possible to correctly represent the same motion by

$$360 \times 60 \times 60 + n + \delta n$$

by adopting a unit of time, $T + \delta T$, such that

$$\frac{T + \delta T}{S} = 1 + \frac{n + \delta n}{360 \times 60 \times 60},$$

subject to the same condition that $360 \times 60 \times 60$ is a mere angle. We should thus have

$$\frac{\delta T}{S} = \frac{\delta n}{360 \times 60 \times 60},$$

and

$$\therefore \quad \frac{\delta T}{T} = \frac{\delta n}{360 \times 60 \times 60 + n},$$

subject to the same conditions; and this is Professor Adams's result.

But if s is the sidereal time, in arc, we adopted up to 1863

(1) $s = a + \omega t = a + (360 \times 60 \times 60 + n)t$;

but after 1864

(2) $s = a' + (\omega + \delta\omega)t' = a' + (360 \times 60 \times 60 + n + \delta n)t'$.

The change of unit therefore between 1863 and 1864 is correctly expressed by

$$\frac{\delta T}{T}=\frac{\delta n}{360\times 60\times 60+n},$$

provided we have introduced no conditions in practice inconsistent with the supposition that $360\times 60\times 60$ in (1) and (2) are mere angles.

But we have assumed that all our tabular quantities which were taken out for a mean noon, r days from epoch, when T was our unit, can be taken out for the rth noon without any correction whatever when we adopt the unit $(T+\delta T)$. In other words, we suppose the two mean noons to be identical, and we thus put

$$360\times 60\times 60\,.\,t=360\times 60\times 60\,.\,t'$$

whenever t' is an integer.

But in this case we must also have

$$nt=(n+\delta n)t',$$

and this would require that

$$\frac{360\times 60\times 60}{n}=\frac{360\times 60\times 60}{n+\delta n}$$

or $n=n+\delta n$ identically, unless the idea of two different units, T and $T+\delta T$, be introduced, as I have done.

It is therefore useless for Professor Adams to assert that he proves me wrong by simply showing inconsistencies between his results, based on an erroneous assumption, and the results which I have obtained and Major-General Tennant also. All our errors are due to the attempt to use an imperfect and unhomogeneous equation, $\omega=360\times 60\times 60+n$, instead of the true equation

$$\omega=\frac{360\times 60\times 60}{t}+n,$$

or if T is the adopted unit and T_0 is the true mean solar solar day,

$$\omega=360\times 60\times 60\,.\,\frac{T}{T_0}+n.$$

This gives at once

$$\delta\omega=360\times 60\times 60\,.\,\frac{\delta n}{n}+\delta n;$$

and it is the neglect of the term $360\times 60\times 60\frac{\delta n}{n}$ which renders it necessary for Prof. Adams to assert that he has nothing to do with the direction of the meridian in space at mean noon, and that my errors are due to my determining the errors of time from the Sun's motion in longitude. This is not strictly the case; but there is no difference between the results obtained from the motion of the meridian or from the Sun's motion in longitude, when the necessary ideas of the difference between the units T and $T+\delta T$, corresponding to the two adopted mean motions n and $n+\delta n$, are introduced into our work.

I need hardly point out that if $n=0$,

$$\omega=\frac{360\times60\times60}{t}+n \text{ becomes } \frac{360\times60\times60}{\text{Sidereal day}},$$

which shows that my result, at all events, satisfies the crucial test proposed by Prof. Adams at the close of his letter.

My form is true for all corresponding values of n, T, and ω. Prof. Adams's form is true only for a single value of n_0, T_0, and ω_0; and being unhomogeneous as regards angular velocities, it cannot be used to correct erroneous values of n and ω by the variations of n and ω considering T as constant.

It may be worth remarking that the form given to ω,

$$360\times60\times60\,.\frac{T}{T_0}+n,$$

is the only form which can satisfy the condition, which in practice is always assumed to be true, that when

$$s=a+(360\times60\times60\,\frac{T}{T_0}+n)t,$$

the principal term, $360\times60\times60\,\frac{T}{T_0}\,.\,t,$

should always be equal to 360° at each true mean noon, for $T\,.\,t=T_0 r$ identically.

Attempts to escape the incidence of my arguments by assuming that we have been measuring motions in terms of t, which is not in itself quantitatively referred to any measurable continuous motion, are hardly worth serious consideration.

It is perhaps worth while to remind those who are interested in these questions, that the alternative of the acceptance of my views of a change in the adopted *tabular* day is the acceptance as a fact that the *real mean solar day* has actually changed to the same amount; for I have shown that the observations of the Moon and the occultations of the stars by the Moon during the whole period 1864–1882 are represented quite as well after the times have been corrected in accordance with my views as they were by Hansen's Tables before the change made in 1864.

Yours truly,
E. J. Stone.

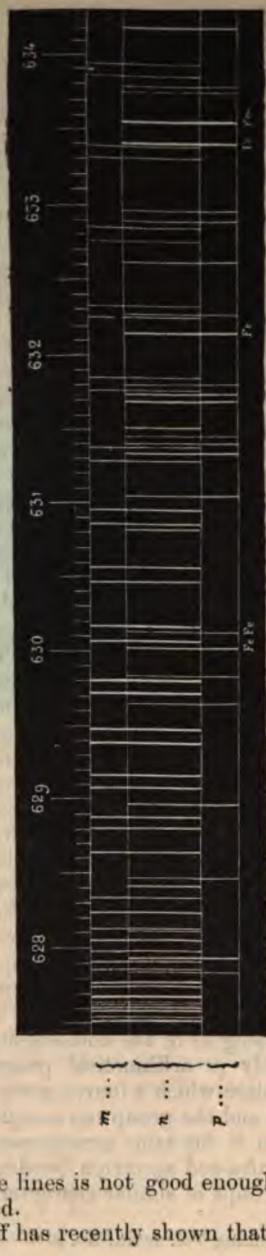
NOTES.

THE TELLURIC LINES α *.—M. Cornu has recently made some important and interesting investigations into the composition of the cluster of lines between the red and orange, the telluric character of which was first recognized by Brewster, and which Angström designated by the letter "α," by which they are generally known. Angström's map being very incomplete in this part of the spectrum, and on too small a scale to properly exhibit its details, M. Cornu's first task was to remap the group on a scale four or five times as great. At first the group appeared to him but a complicated cluster of dark lines irregularly distributed, but when he had succeeded by an ingenious device in separating the lines of solar from those of telluric origin, the latter displayed a most striking regularity in their arrangement. The principle upon which this device depended for its success was one which had been already utilized, but in a different manner, by Prof. Hastings, for the same purpose. In consequence of the rotation of the Sun on its axis, the eastern border is always approaching us, and the western receding from us; the lines in the solar spectrum are therefore slightly displaced towards the blue on the eastern, and towards the red on the western limb, whilst the lines due to the absorption of our atmosphere are not affected. M. Cornu therefore formed a very small image of the Sun on the slit by a lens of about four or five inches focus, and then made the lens oscillate rapidly, so that first one limb of the Sun and then the other was brought up to the slit. The solar lines therefore vibrated to and fro, since they were continually being displaced in one direction or the other as the first or second limb was on the slit, whilst the telluric lines remained stationary.

An essential condition for the success of the experiment was the aplanatism of the spectral image; it was necessary that the lines of the spectrum and the lines perpendicular to them produced by the irregularities of the slit came sharply to focus in the same plane. In this way it was soon ascertained that the telluric lines "form two unequal series of double lines, whose channelled appearance immediately recalls that of the telluric groups A and B." M. Cornu goes on to say that the more the three groups are studied, the more striking does the resemblance between them appear, so that they may be looked upon as forming three harmonic groups, analogous to the triplets of magnesium, zinc, &c. And, further, the reciprocals of the wave-lengths of the homologous lines of the three groups are very nearly in arithmetical progression. The telluric band δ occupies the place which a fourth group should hold; but its lines were too faint and the group too complicated for M. Cornu to be able to detect in it the same arrangement as in A, B, and α. The chart of the infra-red spectrum produced by Capt. Abney seems also to show groups of similar character, but the defi-

* Comptes Rendus, Vol. xcviii. No. 4.

Telluric Group α (Angström).



The scale is double that of Angström's Atlas.

Description.

m (the lines of the complete spectrum prolonged upwards), absorption lines of dry air, the Sun being a few degrees above the horizon.
n, complete spectrum.
p (the lines of the complete spectrum prolonged downwards), lines of solar origin.

The lines attributed to aqueous vapour are those which are not prolonged in either direction.

nition of the lines is not good enough for a decisive agreement to be established.

M. Egoroff has recently shown that the A and B groups are due to the absorption of oxygen. M. Cornu therefore considers it probable that α is likewise due to the same cause, an inference which Prof. Piazzi Smyth had previously been led to make from the "strong family resemblance" between the lines of the low temperature spectrum of oxygen and "the bandlets of lines in this mysterious α band." There are, however, a few lines of terrestrial origin which show, by their disappearance in very cold weather, that they are due to the influence of water vapour.

CHANGES IN THE APPEARANCE OF COMET 1883 *b* (PONS-BROOKS).—Striking as were the early changes in appearance which this remarkable body has exhibited, the alterations it has undergone as it has approached perihelion have been stranger still. The following account is mainly taken from a note by M. Perrotin appearing in the 'Comptes Rendus,' Vol. xcviii. No. 4, whose descriptions have been fully confirmed by several other observers. On Jan. 13 the nucleus, which had resembled a star of the third magnitude the evening and the days preceding, had assumed extraordinary proportions and appeared as a circular disk with a strong reddish-yellow colour about 34″ in diameter, sharply defined at its edge, brightest towards the centre and towards the circumference, and surrounded by a white nebulous halo about 110″ in diameter. The nucleus was in the centre of this disk, which was crossed by two lines of light at an angle of about 40° to each other. These were white, and seemed to stand out in relief on the yellow background of the disk. The brighter of the two seemed to lie in the same direction as the tail. In the obtuse angles formed by these diameters there was a nearly dark space contrasting in a striking manner with the brightness of other parts of this luminous envelope. Lastly, the more condensed part of the nebulosity, which usually surrounded the nucleus for a distance of 3′, had lost its intensity in a remarkable manner. By the following day the comet had resumed its ordinary appearance, and it remained without noteworthy change until Jan. 19, when it presented the same general appearance as on the 13th, with the difference that the central disk was slightly lengthened at right angles to the direction of the tail, the two bright diameters were perpendicular to each other, the dark region was narrower and darker, and the outer halo fainter. At the time of these changes the spectroscope showed that the three ordinary cometary bands were much brighter than usual, and that the disk gave a continuous spectrum which was very bright in the red, and which also seemed to indicate the presence of sodium. The violet hydrocarbon band was also made out.

DANISH EXPEDITION FOR OBSERVATION OF THE TRANSIT OF VENUS.—Dr. Pechüle has recently published an account of the Danish expedition for observation of the Transit of Venus. Finding a Brazilian party at St. Thomas, where he had primarily intended to observe the transit, Dr. Pechüle selected as his station Bülowsminde, in the island of St. Croix, and began the necessary observations on Nov. 16. The observations of Dec. 5 and 6 are printed in detail in the report, as are also those of the transit. The time of first contact at ingress according to Dr. Pechüle was $2^h\ 12^m\ 6^s$ Paris mean time, and by Mr. Hovgaard $2^h\ 11^m\ 9^s$; but the latter observer had some doubt about the minute, and it is considered that Mr. Hovgaard's time is one minute early. Both observers remarked a very bright ring round the edge of the planet not on the Sun. Just before internal contact Mr. Hovgaard saw a black drop. Unfortunately clouds prevented the observations of internal contact; but Dr. Pechüle made several sets of measures for determination of difference of R.A. between the centres of the Sun and Venus, when the state of the sky permitted.

During the time the observers remained at Bülowsminde after the transit, Dr. Pechüle undertook a spectroscopic examination of the southern stars visible at St. Croix at the time of his stay down to the fifth or sixth magnitude and of the red stars in Birmingham and Schjellerup's catalogues, 568 stars being examined in the first class, and 68 in the second. Dr. Pechüle has preferred to follow Secchi and D'Arrest in his classification of spectra rather than Vögel, who groups together Secchi's third and fourth classes as two sections of class III. A seventh-magnitude star with unusual spectrum was noticed 15′ north of o' Canis Majoris. The less refrangible part of the spectrum was only indistinctly interrupted, and was a little brighter towards the red, then, after a wide dark interval, came a very bright narrow zone, which faded rapidly towards the more refrangible edge and forms the end of the spectrum. The star was of a bluish colour; it has been examined by Prof. Pickering, who has noted bright lines. An examination of the southern sky for comets was fruitless. The telescope used was a 6-inch, and the spectroscope was of very small dispersion. The position of Bülowsminde is longitude $4^\circ\ 18^m\ 55^s{\cdot}0$ W. of Greenwich, and Latitude $17^\circ\ 44'\ 43''$ N.

Dr. Pechüle also discusses the distribution of the red stars in the southern heavens, and is of opinion that they seem to be more numerous as the Milky Way is approached, and to be most numerous within it; and this not merely in the same proportion as other stars, a relation which Dr. Doberck has shown seems to be indicated in the northern hemisphere, but the proportion of the red stars to others increases with nearness to the Galaxy.

Wave-lengths of A, *a*, and of some Prominent Lines in the Infra-red.—Capt. Abney, at the Meeting of the Royal Society on 1883, Dec. 6, criticised the map of the solar spectrum from C to A recently brought out by M. Fievez. He finds that his photographs do not agree with M. Fievez's chart in the details of the grouping of the *a* group, and in the interval between *a* and A, and that A in M. Fievez's map is wanting in some details shown in the photographs. Comparison photographs of the first order of the red with the second order of the ultra-violet, or of the second order of the red with the third order of the green gave wave-lengths from the lines from *a* to A which did not agree with those given by Fievez.

Description of line.	λ from comparison of 1st and 2nd orders.	λ from comparison of 2nd and 3rd orders.	λ according to Fievez.	Remarks.
a	7184·4 7185·4	7184·5 7185·4	7197·7 7198·7	This is shown in Angström's map as a single line, λ 7184·9.
Most refrangible edge of A	7593·6	7593·7	7600·0	Angström gives 7604 for the centre of this line: which of the bands he took as A is not clear. Langley gave 7600·9 for this edge.
Centre of 6th pair of lines in the flutings following A	7644·2	7644·33	7652·2	

The above wave-lengths Capt. Abney considers as accurate as those of the maps from which they were taken, Cornu's being used for the ultra-violet, Angström's for the green.

The following are wave-lengths of some of the principal lines in the infra-red, the scale-numbers being those of Capt. Abney's map of the infra-red published in the Phil. Trans. part ii. 1880:—

Scale-number.	Description.	λ from comparison of 1st and 2nd orders.	λ from comparison of 2nd and 3rd orders.
1046	Double line, of which the components have the accompanying wave-lengths	8226·4 8229·9	8226·3 8229·9
1441	....	8496·8	8496·8
1509	....	8540·6	8540·7
1685	....	8661·0	8661·0
2175	Double line, the components of which have the accompanying wave-lengths	8986·2 8989·5	
2638	Ditto	9494·5 9500·1	
3161	....	9633·8	9633·9

Prof. Rowland's concave gratings were employed for this comparison.

The Secular Acceleration of the Moon's Mean Motion.—Prof. Th. v. Oppolzer has recently published a paper ('Astronomische Nachrichten,' No. 2573) in which he endeavours to account for the difference between the theoretical and the observed values of the secular acceleration of the Moon's mean motion. After commenting on the unsatisfactory nature of Delaunay's explanation of this phenomenon depending on the action of the tides, Prof. Oppolzer goes on to state his own views, which are based on the generally admitted existence of finely divided cosmical matter pervading space, the larger particles of which, striking our atmosphere, reveal themselves as meteors. It is by the action of this cosmical matter on the motions of the Earth and Moon that Prof. Oppolzer proposes to account for the discrepancy referred to above. Three ways in which the presence of this matter (the individual particles of which are supposed to possess no elasticity) will influence the Moon's apparent motion are particularized, these alone being taken into consideration in this first mere approximation to a result deduced from Prof. Oppolzer's theory:—1. The mass of the Earth and also of the Moon increases through the accumulation of cosmical dust on them, and there results in the mean longitude of the Moon a term multiplied by t^2. 2. A portion of the matter received by the Moon diminishes its tangential velocity, and thus produces in the mean longitude of the Moon a term also multiplied by t^2. 3. The matter which accumulates on the Earth alters its velocity of rotation and therefore its period, which variation of period, being transferred to the Moon's motion, produces a term in the mean longitude multiplied by t^2.

By quite approximate formulæ and by some assumptions which are perhaps sufficiently accurate for the present purpose, Prof. Oppolzer computes the secular acceleration in each of these three cases, and deduces a result which contains known coefficients and an unknown quantity h, the height of a layer of the cosmical matter which will accumulate on the Earth's surface in a century. Putting approximate values for the known coefficients, there result for the three cases specified above respectively,

$$\Delta L_1 = +0''\cdot 87ht^2, \quad \Delta L_2 = +0^{\circ}\cdot 26ht^2, \quad \text{and } \Delta L_3 = +0''\cdot 68ht^2,$$

and by adding these the total acceleration is $+1''\cdot 81ht^2$ (where the unit of h is the millimetre and of t a Julian century). As the secular acceleration from observation is about 11″, and that derived from theory about 6″, it would suffice that the precipitation of cosmical dust on the Earth's surface should amount to 2·8 millimetres in height in a century, to account for the difference between theory and observation, an amount which cannot be considered as beyond the limits of possibility. Prof. Oppolzer finds that the density of the medium through which the Earth moves (on the hypothesis that $h = 2\cdot 8$ millimetres) is $\frac{\text{Density of air}}{3,760,000,000,000}$.

A. M. W. D.

The Royal Observatory, Brussels.—In consequence of M. Houzeau's resignation of the directorship, a committee consisting of MM. Liagre, Mailly, and Stas has been appointed to preside over the Observatory, and the following appointments have been made:—M. Niesten has been appointed chief of the department of mathematical astronomy, M. C. Fievez is temporarily entrusted with the direction of the physical department, and with M. Lagrange he has been promoted from the rank of assistant to that of astronomer, and M. Vincent has been promoted from being assistant meteorologist to be meteorologist. Vol. iv. of the 'Annals' (new series) has just been published. In addition to the meridian observations for 1879–80 and 81, it contains drawings of the Moon, observations of Jupiter's satellites observed in 1880, physical observations of Jupiter and of comets (*b*) and (*c*) 1881, and a study of the solar spectrum. The 'Annuaire' for 1884, in addition to the usual ephemerides, contains articles on pure and applied science, the Belgian observations of the Transit of Venus, minor planets and comets discovered in 1883, and Belgian rainfall and tides.

The Dusky Ring of Saturn.—On 1883, October 29, Prof. Davidson observing Saturn with a 6·4-inch equatoreal, under exceptionally favourable atmospheric conditions, noted that with a moderate power the division of the ring was visible all round. With a higher power the inner edge of the dusky ring was "wonderfully well defined," and the division of the outer ring was made out further than in the well-known Cambridge drawing. The shadow of the planet on the ring was so well-defined that the irregularity at the subdivision of the ring was unmistakable. The markings and colours on the planet came out with very unusual distinctness; but one of the best revealed features was the undoubted difference in brightness of the dusky ring at the two ansæ, the preceding part being decidedly brighter than the following ansa, a peculiarity which was noted with different eyepieces and also by another observer.

The Movement of Mimas *.—M. Bailland has investigated the motion of Mimas and believes that he has established two periodic inequalities in it. The coefficient in each case is nearly 8°, and the periods are 300 days and 5 years respectively. The two inequalities were each at its minimum at the same time in 1882 July. The mean longitude of Mimas is sensibly represented by the following formula:—

$$L = nt + 30^\circ - 8^\circ \cos \tfrac{6}{5}(t - T) - 8^\circ \cos \tfrac{1}{5}(t - T),$$

where T corresponds to 1882 July.

A New Astronomical Journal.—The first number of a monthly journal entitled 'Bulletin Astronomique,' and brought out under the auspices of the Paris Observatory, appeared on Feb. 15. It is edited by M. Tisserand, assisted by MM. Bigourdan, O. Callandreau, and R. Radan.

* Comptes Rendus, Vol. xcviii. No. 4.

THE LATE PROF. J. F. J. SCHMIDT.—Astronomers will learn with regret the death of Prof. Johann F. Julius Schmidt, for many years Director of the Observatory at Athens, and an associate of the R. A. S. since 1874. The King and Queen of Greece were present at the Observatory during the delivery of the funeral oration. The funeral was of a public character.

THE GLASGOW CATALOGUE OF STARS.—The catalogue, which for some years past has been in active preparation at the Glasgow Observatory has just been published by Prof. Grant. The Royal Society has contributed largely towards the expense of printing from the Government Grant Fund. The catalogue contains 6415 stars, the observations extending over the years 1860 to 1881.

U GEMINORUM.—Mr. G. Knott observed the recent maximum of the variable star U Geminorum at Cuckfield. His estimates of magnitude were—Jan. $24^d\ 8^h\ 10^m$, 13·3; $26^d\ 9^h\ 50^m$, 9·6; $27^d\ 8^h\ 53^m$, 9·7; $28^d\ 8^h\ 15^m$, 9·95; $30^d\ 9^h\ 0^m$, 11·4; and Feb. $2^d\ 7^h\ 20^m$, 13·9. On Jan. 25 observations were prevented by clouds; but it is possible that the maximum occurred on that day, as in 1877 the star increased in magnitude from 13·2 to 9·8 in $26^h\ 20^m$. Mr. Knott observed a maximum on 1883 Jan. 30, which was corroborated by the observations of M. Safarik. The limits of magnitude are from $14\frac{1}{2}$ to $9\frac{1}{4}$ of Argelander's scale, and the period has been found by Mr. Knott to vary from 71 to 126 days.

THE SATELLITES OF MARS.—Deimos, the outer satellite of Mars, has, we hear, been observed during the present opposition of the planet by Prof. Asaph Hall, notwithstanding the fact that the planet was near aphelion. It would therefore appear that this satellite is observable with very large telescopes at every opposition.

IT is stated that Mr. Clement L. Wragge intends establishing an astronomical and meteorological observatory at a house on Stephens Terrace, Gilberton, near Adelaide. The meteorological instruments comprise mercurial barometers, a barograph, self-registering and other thermometers, rain-gauges, ozone tests, a rainband spectroscope, and other instruments used by Mr. Wragge upon Ben Nevis. Mr. Wragge will train an assistant to perform the work during his absence.

THE international conference to consider the unification of longitudes and time has been called to meet in Washington on 1884, Oct. 1.

THE JANUARY NUMBER OF THE 'OBSERVATORY.'—We learn that several American subscribers have failed to receive the January number of the 'Observatory.' It was mailed as usual; but should any Subscriber not have received a copy, another will be forwarded on his intimating the fact.

Approximate Times of Transit of the great Red Spot and Equatoreal White Spot across the Central Meridian of Jupiter in March 1884.

Day of Month.	Red Spot on C.M.	White Spot on C.M.	Day of Month.	Red Spot on C.M.	White Spot on C.M.
	h	h		h	h
1.	9·2	10·6	17.	12·4	10·2
2.	15·0	6·3	18.	8·2	5·9
3.	10·8	11·8	19.	14·0	11·4
4.	6·7	7·5	20.	9·9	7·1
5.	12·5	13·0	21.	5·7	12·5
6.	8·3	8·6	22.	11·5	8·2
7.	14·1	14·2	23.	7·4	13·7
8.	10·0	9·8	24.	13·2	9·4
9.	15·7	15·4	25.	9·0	14·9
10.	11·6	11·0	26.	14·8	10·6
11.	7·5	6·7	27.	10·6	6·3
12.	13·2	12·2	28.	6·5	11·8
13.	9·1	7·9	29.	12·3	7·5
14.	14·9	13·3	30.	8·2	13·0
15.	10·7	9·0	31.	13·9	8·6
16.	6·6	14·5	Apr. 1.	9·8	14·2

Further observations of the red spot have been obtained at Bristol as under:—

	On C.M.	Long. (870°·42).
1884, January 27........	$11^h\ 10^m$	82°·6
February 6........	9 29	86 ·7
„ 9........	6 58	86 ·9
„ 14........	6 5	87 ·3

The equatoreal white spot was seen in transit as follows:—

	On C.M.	Long. (878°·46).
1884, January 27......	$10^h\ 23^m$	79°·3
February 14	11 16	85 ·5
„ 15......	6 56	85 ·4

The increasing longitude shown by this object proves that its retarded motion has been continued.

This white spot is certainly much less conspicuous than formerly; its obscurations are more frequent, so that there is considerable difficulty in identifying it from other white spots in the same latitude. Its slackening velocity will cause it to be presented on the C.M. a little later than the time indicated in the table. It will occupy nearly coincident longitude with the red spot on March 8, and at about 10^h the two objects will pass the central meridian together. Their relative positions should be very carefully noted on this occasion. If the red spot cannot be seen with sufficient distinctness to allow safe comparisons, then it will be advisable to note the exact place of the white spot relatively to the hollow in the great southern dark belt, which is an object of such prominent character as to be readily made out in very small instruments.

Bristol, 1884, Feb. W. F. DENNING.

Differences of Right Ascension and Declination between Iapetus and the centre of Saturn. By A. MARTH*.

		$\alpha-A.$	$\delta-D.$			$\alpha-A.$	$\delta-D.$
		s	"			s	"
Mar.	1	+27·2	− 18	Mar.	13	+36·8	+143
	2	+29·1	− 3		14	+36·3	+153
	3	+30·8	+ 12		15	+35·6	+162
	4	+32·3	+ 27		16	+34·7	+169
	5	+33·6	+ 42		17	+33·6	+176
	6	+34·7	+ 56		18	+32·3	+182
	7	+35·7	+ 71		19	+30·8	+187
	8	+36·4	+ 84		20	+29·2	+190
	9	+36·9	+ 97		21	+27·4	+193
	10	+37·2	+110		22	+25·5	+195
	11	+37·3	+122		23	+23·4	+195
	12	+37·2	+133		24	+21·3	+194

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

MIMAS (East).			ENCELADUS.			TETHYS.			DIONE.		
		h			h			h			h
Mar.	3	11·3	Mar.	1	15·4	Mar.	2	12·9	Mar.	3	10·0
	4	9·9		3	0·3		4	10·3		6	3·7
	5	8·5		4	9·2		6	7·7		8	21·2
	6	7·1		5	18·1		8	5·0		11	14·9
				7	3·0		10	2·3		14	8·9
MIMAS (West)				8	11·9		11	23·4		17	2·6
Mar.	11	11·4		9	20·8		13	20·9		19	20·1
	12	10·0		11	5·7		15	18·2		22	13·8
	13	8·6		12	14·6		17	15·6		25	7·8
	14	7·2		13	23·5		19	12·8			
				15	8·4		21	10·2	RHEA.		
HYPERION.				16	17·3		23	7·6	Mar.	2	7·5
Feb.	25	9·4		18	2·2					6	20·0
Mar.	17	15·0		19	11·1	TITAN.				11	8·6
				20	20·0	Mar.	6	7·2		15	21·1
				22	4·9		22	5·2		20	9·7
				23	13·8					24	22·2
				24	22·7						

* 'Monthly Notices,' Vol. xliii. No. 8.

The following table will furnish the means for inferring approximate distances and position-angles of the satellites from the diagram given in Plate I., opposite page 26.

	Position-angle of Minor Axis of Ring.		Semi-major axis of orbit of Titan.
Feb. 23	356° 40′	Feb. 23	187″·0
Mar. 4	356 37	Mar. 4	183 ·6
14	356 32	14	180 ·5
24	356 26	24	177 ·6

Astronomical Memoranda, 1884, *March.*

Sun. Mar. 1, sets 5^h 39^m, rises 18^h 46^m; Mar. 11, sets 5^h 56^m, rises 18^h 22^m; Mar. 21, sets 6^h 13^m, rises 17^h 59^m; Mar. 31, sets 6^h 30^m, rises 17^h 36^m.

Equation of Time:—Sun *after* Clock, Mar. 1, 12^m 25^s; Mar. 11, 10^m 1^s; Mar. 21, 7^m 7^s; Mar. 31, 4^m 4^s.

Sidereal Time at Mean Noon:—Mar. 1, 22^h $38^m{\cdot}8$; Mar. 11, 23^h $18^m{\cdot}2$; Mar. 21, 23^h $57^m{\cdot}6$; Mar. 31, 0^h $37^m{\cdot}1$.

Moon.	*sets.* h	m		*sets.* h	m		*rises.* h	m
Mar. 1	10	43	Mar. 11	18	17	Mar. 21	15	16
2	11	59	12	18	41	22	15	49
3	13	8	13	*rises.*		23	16	19
4	14	11	14	9	16	24	16	49
5	15	4	15	10	19	25	17	17
6	15	51	16	11	20	26	17	44
7	16	29	17	12	15	27	18	17
8	17	0	18	13	8	28	18	50
9	17	28	19	13	55	29	*sets.*	
10	17	54	20	14	38	30	10	56
						31	12	2

First Quarter, Mar. 4, 1^h 33^m; Full Moon, Mar. 11, 7^h 40^m; Last Quarter, Mar. 19, 11^h 13^m; New Moon, Mar. 26, 17^h 47^m.

Mercury is in superior conjunction Mar. 29^d 20^h, and is unfavourably situated for observation throughout the month.

Venus is an evening star throughout the month. Diameter:—Mar. 1, 14″·2; Mar. 31, 17″·2. Illuminated portion of disk, 0·721 on Mar. 15.

Mar. 1,	R.A. 1^h $10^m{\cdot}8$,	Dec. 7° 33′ N.,	tr. 2^h 32^m,	sets 9^h 13^m
31,	3 24 ·6,	20 40 N.,	2 48	10 45

Mars near the Præsepe is stationary Mar. 12^d 19^h. Diameter:—Mar. 1, 14″·4; Mar. 31, 11″·2. Illuminated portion of disk 0·940 on Mar. 15.

Mar. 1,	R.A. 8^h $26^m{\cdot}3$,	Dec. 23° 19′ N.,	tr. 9^h 46^m,	sets 18^h 3^m
31,	8 30 ·3,	21 59 N.,	7 52	15 58

Jupiter between Cancer and Gemini, stationary Mar. 19ᵈ 22ʰ. Diameter: —Mar. 1, 40″·4; Mar. 31, 37″·2.

Mar. 1,	R.A. 7ʰ 48ᵐ·5,	Dec. 21° 44′ N.,	tr. 9ʰ 8ᵐ,	sets 17ʰ 13ᵐ
31,	7 46 ·9,	21 48 N.,	7 9	15 14

Saturn is nearly stationary in Taurus.

Mar. 1,	R.A. 4ʰ 8ᵐ·8,	Dec. 19° 18′ N.,	tr. 5ʰ 29ᵐ,	sets 13ʰ 19ᵐ
31,	4 17 ·7,	19 48 N.,	3 40	11 32

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Mar. 14	40″·14	17″·43	26″·69	11″·59	16″·00

The south side of the rings is visible, the elevation of the Earth above their plane on Mar. 14 being 25° 45′, and of the Sun 26° 15′.

Uranus is near β Virginis, in opposition to the Sun Mar. 15ᵈ 18ʰ.

Mar. 1,	R.A. 11ʰ 49ᵐ·3,	Dec. 2° 2′ N.,	tr. 13ʰ 8ᵐ,	rises 6ʰ 54ᵐ
31,	11 44 ·6,	2 32 N.,	11 6	4 50

Neptune:

Mar. 1,	R.A. 3ʰ 6ᵐ·5,	Dec. 15° 42′ N.,	tr. 4ʰ 27ᵐ,	sets 11ʰ 54ᵐ
31,	3 9 ·5,	15 56 N.,	2 32	10 0

Partial eclipse of the Sun on Mar. 26, invisible at Greenwich. Begins on the Earth generally Mar. 26, 17ʰ 10ᵐ·8 G.M.T. in Long. 9° 51′ E. and Lat. 53° 43′ N. Greatest eclipse Mar. 26, 18ʰ 2ᵐ·2 in Long. 7° 48′ W., Lat. 72° 4′ N., about one-seventh of the Sun's diameter being obscured. Ends on the Earth generally Mar. 26, 18ʰ 53ᵐ·5, in Long. 115° 29′ W., Lat. 87° 9′ N.

Phenomena.

	G. M. T. h m	
Mar. 1	10 2	J. i. Tr. I.
	12 22	J. i. Tr. E.
2	7 10	J. i. Oc. D.
	7 51	J. iii. Ec. R.
	10 24	J. i. Ec. R.
3	6 49	J. i. Tr. E.
	10 44	δ¹ Tauri Oc. D. 115°.
	11 10	δ² Tauri Oc. D. 93°.
4	9 48	J. iv. Ec. D.
	12 51	115 Tauri Oc. D. 46°.
	13 32	J. ii. Tr. I.
	14 15	J. iv. Ec. R.
5	14 24	J. iii. Tr. I.
Mar. 6	8 36	J. ii. Oc. D.
	10 10	λ Geminorum Oc. D. 107°.
	11 11	λ Geminorum Oc. R. 226°.
	13 27	J. ii. Ec. R.
7	14 32	J. i. Oc. D.
8	10 18	κ Cancri Oc. D. 47°.
	11 51	J. i. Tr. I.
	14 11	J. i. Tr. E.
9	7 41	J. iii. Oc. R.
	8 23	J. iii. Ec. D.
	8 59	J. i. Oc. D.
	11 52	J. iii. Ec. R.
	12 19	J. i. Ec. R.

Phenomena (continued).

	G. M. T. h m	
Mar 9	16 7	16 Sextantis Oc. D. 100°.
10	6 13	34 Sextantis Oc. D. 44°.
	8 38	J. i. Tr. E.
11	6 48	J. i. Ec. R.
	9 22	*v* Leonis Oc. D. 98°.
	10 25	*v* Leonis Oc. R. 213°.
12	7 41	J. iv. Tr. I.
	12 8	J. iv. Tr. E.
13	11 0	J. ii. Oc. D.
15	8 5	J. ii. Tr. E.
	13 41	J. i. Tr. I.
16	7 47	J. iii. Oc. D.
	10 49	J. i. Oc. D.
	11 20	J. iii. Oc. R.
	12 24	J. iii. Ec. D.

	G. M. T. h m	
Mar. 16	14 14	J. i. Ec. R.
17	8 8	J. i. Tr. I.
	10 28	J. i. Tr. E.
18	8 43	J. i. Ec. R.
20	13 27	J. ii. Oc. D.
21	8 22	J. iv. Ec. R.
22	7 39	J. ii. Tr. I.
	10 34	J. ii. Tr. E.
23	11 30	J. iii. Oc. D.
	12 40	J. i. Oc. D.
24	7 56	J. ii. Ec. R.
	9 59	J. i. Tr. I.
	12 19	J. i. Tr. E.
25	10 38	J. i. Ec. R.
29	10 11	J. ii. Tr. I.
	13 5	J. ii. Tr. E.
31	10 31	J. ii. Ec. R.
	11 52	J. i. Tr. I.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

Publications received:—Annuaire du Bureau des Longitudes pour l'an 1884 (Gauthier-Villars, Paris)—Arthur Searle, *The Zodiacal Light* (Proc. American Acad.)—S. W. Burnham, *Double Star Observations made at the Dearborn Observatory, Chicago, in* 1879 *and* 1880 (Mem. Royal Astron. Soc.)—*Annuaire de l'Observatoire Royale de Bruxelles*, 1884, 51*st year* (F. Hayez, Bruxelles)—*Washington Astronomical and Meteorological Observations for* 1879 (Government Press, Washington)—Clark, *Transit Tables for* 1884, and *A Manual of the Transit Instrument* (Spon, London)—*Liverpool Astronomical Society, Abstracts of Proceedings*—H. C. Russell, F.R.A.S., *The Sydney Observatory History and Progress, Anniversary Address to the Royal Soc. of N. S. Wales* (delivered 1882, May 3), *The Spectrum and Appearance of the recent Comet* (b) 1881 (Royal Society of New South Wales), and *Results of Rain and River Observations made in N. S. Wales during* 1882 (Richards, Government Printer, Sydney)—Prof. Pritchard, F.R.S., *Photometric Determination of the relative brightness of the brighter Stars north of the Equator* (Mem. R. A. S. vol. xlvii.)—Elias Loomis, *Contributions to Meteorology* (19*th paper*)—*Report of the Council of the National Academy of Sciences, Washington—Weather Report for October*, 1883 (*Jamaica*)—Prof. D. Kirkwood, *The Zone of Asteroides and the Rings of Saturn* (Proc. Amer. Philos. Soc. xxi. 114).

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THE OBSERVATORY,

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No. 84. APRIL 1. 1884.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1884, March 14.

E. Dunkin, F.R.S., *President*, in the Chair.

Secretaries: E. B. Knobel, and
Lieut.-Col. G. L. Tupman.

The Minutes of the last Meeting were confirmed.

Lieut.-Col. Tupman announced that 87 presents had been received since the last Meeting. Amongst these were a six-prism spectroscope, presented by Mr. J. D. Perrins; a portrait of Dr. Kitchener, presented by Mr. Lecky; and the Glasgow catalogue of stars, presented by Professor Grant, the catalogue being one of the finest of our modern catalogues.

The thanks of the Society were unanimously voted to the respective donors.

Mr. Downing read a paper "On an instance of change of personality in observing position-angles of Double-Stars, and on the Orbit of α Centauri." He said that he had come across some remarkable results in reference to the change in the method of observing the position-angles of double stars in the observations of α Centauri. He had endeavoured to correct the elements of the orbit deduced by Dr. Elkin by observations made at Sydney and also by those made by Messrs. Maxwell Hall, Ellery, and Tebbutt. Dr. Elkin's orbit represented the observations to the end of 1877 very accurately; but he thought he should be able to correct them by discussing later observations. He found the position-angles computed from Dr. Elkin's elements would require corrections of $7\frac{1}{2}°$ for 1878, and $3\frac{1}{4}°$ for 1880 and 1881 to make them agree with the observed position-angles. After working for some time he determined to omit the Sydney observations, which differed considerably from the computed places, and from other observations made at about the same time, and were on that account possibly erroneous. He therefore determined the corrections to Dr. Elkin's elements from observations made by Ellery, Hall, and Tebbutt alone; the corrections then would be about 1° or 1¼° to the computed position-angles. He had given a detailed comparison of places computed from the corrected elements with the observations made since 1870·0, and it appeared that Russell's position-angles agreed with the observations of other astronomers up to the end of 1877, but there appeared discordances of 5° or 6° in 1878, and 3° or 4° in 1880. This indicated some change in Russell's method of observation, probably depending on change of direction of the line joining the components. The elements given in this paper were not entitled to very great weight, as they depended only on a small number of observations; but he had brought forward the paper chiefly to point out the change in method of observing referred to above.

Dr. Gill. I have been a good deal mixed up with α Centauri of late, having had occasion to look into the matter with Dr. Elkin, who has in preparation a work on the orbit of α Centauri which is tolerably exhaustive. I have seen a graphical representation which he has prepared to represent the whole of the observations made of this star. The large systematic errors dependent on the angle which the position-angle of the double star makes with the vertical are most extraordinary. The separate observations even by the same observer differ so widely as even at times to have given rise to the idea that α Centauri is not moving according to the laws of gravitation. But upon the whole the observations are very fairly represented. Such cases will not be found merely in Russell's observations, but in Jacob's and those of everybody who has observed this star. I am sorry Mr. Downing should have devoted so much time to this, until the Cape observations have been published; we have now many hundreds of observations ready for the press and they will soon appear.

Mr. A. Common read a paper containing "suggestions for improvements in the construction of large Transit-circles." The ordinary form of transit instrument was not the best as an astronomical instrument. Its chief defect was the flexure of the tube carrying the optical portions, and in a less degree of the axis itself. If the movement was parallel to the observer the observation would be but slightly affected; but when the instrument was made into a transit-circle the flexure affected the line of vision. The larger and smaller transit-circles differed only in weight; rigidity had been expected from an additional weight of metal. Mr. Common then went on to describe the usual form of the transit-circle, illustrating it by drawing on the blackboard, and also described by the same means the improvements he proposed, which consisted mainly in strengthening the supports carrying the tube by varying their shape, and in using the mercury troughs to sustain the weight of the tube when in certain positions. By these means he believed that flexure would be practically eliminated.

The Astronomer Royal. I am inclined to agree with Mr. Common that some improvement is desirable in the transit-circle to diminish or prevent flexure. I cannot profess to enter into a criticism of his plan, which requires some study; but it appears to me to be a new departure which may possess great advantages. I think, with Mr. Common, that it is a mistake to have a central cube, which is a form that may introduce errors resulting from its weakness. The plan Mr. Common proposes appears to be rather a return to the form of Troughton's mural circle, an instrument which seems to me well contrived for measuring zenith-distances, though not adapted for determining right ascensions. When I was consulted as to the construction of the new transit-circle for the Melbourne Observatory, I was so convinced of the difficulty of this form of cube that I suggested to Mr. Simms to introduce something of the principle of the mural circle. I did not feel at liberty to propose a radical change; but I got Mr. Simms to make the central portion in a barrel form so as to strengthen it, the object-glass and eyepiece tubes being attached to the two ends of the barrel, though I considered the perforations in the central portion for viewing the collimators and for illumination as still objectionable. So far as I am concerned, I am inclined to think Mr. Common's view is the correct one, and that his plan will be a great improvement. As far as I understand, the construction really corresponds with that of the mural circle.

Mr. Common. Very nearly.

The Astronomer Royal. The object-glass and wires can be attached at any point, so that the telescope can be fixed with the circle. That was the principle of the mural circle. It depended on the attachment of the object-glass to one limb of the circle, and the wires to the other limb, with the tube connecting the two passing through the centre; but the instrument did not depend upon the stiffness of the tube.

Dr. Gill. There is no tube in this of Mr. Common's?

Mr. Common. No.

The Astronomer Royal. In the mural circle the tube was not essential.

Mr. Common. There are objections to the tube.

The Astronomer Royal. The difficulty we had to contend with in the Melbourne instrument was that apertures had to be made in the central part for illuminating purposes; and at that time it occurred to me that the direction in which we should look for improvement would be in illuminating from the object-glass, and dispensing with any piercing of the central portion.

Mr. Stone. As far as my experience goes, I have met with two serious cases of what may be called flexure; but in both cases the greater part of the flexure did not arise from the tube at all, but from the object-glass and the object-glass cells; and I am rather inclined to think that has been so in other cases. Certainly in regard to the transit-circle at the Cape there was a looseness of the attachment of the two lenses of the object-glass which gave rise to trouble, and the same thing occurred in the transit-circle at Oxford. There was a large flexure in the transit-circle, and it was naturally supposed to be due to the flexure Mr. Common has spoken of, and we strengthened the tube, but without effect. It then occurred to me the fault was in a looseness of the cap or the object-glass. There was nothing of that kind in the object-glass at Oxford, but the object-glass was screwed on and the flexure was found to be in the screws. So that I fear that supposing you do your best to strengthen these tubes, you will still find there may be faults in the fitting of the object-glass, which was the case in the instances I have named. Of course the method Mr. Common has recommended of taking off the friction by mercury is, I have no hesitation in saying, a step in the right direction; but if you multiply all these means of testing you will find that the practical observer gets into bewilderment; yet it is no doubt desirable that these things should be brought out and recorded; but I am not so sure that they will lead to so great a practical improvement as is supposed, because I feel sure the flexure is due as much to the object-glass, and the way in which the lenses are attached to each other, as to the tube itself.

Mr. De La Rue. Mr. Stone has described a defect in the object-glass itself. That requires to be dealt with in a different way to that which Mr. Common has proposed in regard to the flexure of the instrument. The springs that hold the object-glass in position are not sufficiently strong; or there may be defects in the cell, or there may be a tilting of the object-glass, or the eyepiece also may be wrong; but these must be dealt with on lines different to those laid down by Mr. Common for an improvement of the transit-circle. The details will of course have to be worked out; but I think that Mr. Common has hit a main blot in the construction of transit-instruments.

Mr. Common said the question of the object-glass did not enter into this subject; but he had dealt with object-glasses in a paper read some time ago.

The President. I am sure we are much obliged to Mr. Common for this paper. I am certain the subject is one of considerable importance to all astronomical observers, particularly those connected with meridional observations. Every observer on the meridian knows that we are troubled very much with small discordances in the results. These are caused by a variety of circumstances, one of which is the effect of the flexure of the telescope. In observatories we do what we can to get rid of discordances caused by flexure, by observing stars by reflection and direct vision, which enables us in some measure to correct some of the discordances that may be due to flexure; but at the same time if any plan can be suggested by which we may get rid of flexure, it will be of very high importance indeed. Of course, as Mr. Stone said, if the object-glass is not fixed firmly there will be flexure; but in all good telescopes the object-glass ought to be firmly fixed. The flexure which observers notice is, I think, chiefly due to the bending of the tube of the telescope. Anything that can be done to obviate that will be of great service.

A vote of thanks was given to Mr. Common for his paper.

Dr. Gill. I have no paper to read in the ordinary sense of the word, but as the Secretaries have expressed a desire that I should give an account of the work in which I have been engaged for the past five years at the Cape, I have put down a few notes in reference to it. Our work has been of two kinds: first, the reduction of old observations, and, secondly, new investigations. My predecessor, Mr. Stone, did an enormous amount of work in clearing up arrears of observations, reducing them, and publishing them in a systematic form in the 1840 and 1860 Catalogues; but amongst other things there remained behind a number of meridional observations made with the transit-instrument and mural circle between 1849 and 1852. Such time as he could spare from his other labours, Mr. Stone devoted to the production of these Catalogues, and I found the work of computation about half-finished when I arrived at the Cape; and I carried out the work from time to time as I could, on the same principle as Mr. Stone had adopted. The mere work of reduction was but a trifle compared with the final work of revision and comparison with other catalogues and the discussion which was involved. No one could imagine the labour involved in the construction of these Catalogues because of the failings of two of the observers; but it is, I am happy to say, now completed, and in the hands of Her Majesty's Stationery Office for publication. It consists of over 4800 stars, including nearly all the stars south of the equator in the British Association Catalogue, and is reduced to the equinox of 1850. The point, however, that I have in view in bringing this Catalogue forward is singularly *à propos* of Mr. Common's paper just read. I have reduced all the stars which are common to Mr. Stone's Catalogue of 1880 back to 1850, though of course there must be considerable accidental uncertainty in the proper motions of those stars. As there are 3600 stars common to both Catalogues, the errors due to proper motion will be very inconsiderable in the mean; and when these reductions have been made there comes out a very extraordinary systematic difference between the two Catalogues. On the equator and at the pole, both the Catalogues are practically identical, but from the equator to the pole there is a regular curve of error in right ascension. At about 10° S. of the zenith this difference is no less than $0^{s}\cdot15$, corresponding to a second of arc at the equator. A second of arc is a quantity which is very much in excess of the error of one single observation. Now what can be the origin of this? I think this difference raises a question of serious importance in connection with the equipment of our observatories. The observations in question were made with a transit-instrument by Dollond. It was a reversible instrument, and was reversed frequently, and the levellings were generally done by Sir Thomas Maclear, and the level was determined both by a spirit-level applied to the pivots and by reflection from mercury; but still, notwithstanding all we could do to find out the errors, this difference having a maximum at 10° S.Z.D. occurred between the pole and the equator. Now in 1850 the Greenwich observations were made with a precisely similar reversible instrument, and we have compared the catalogue of 1850 with the present Catalogue, and the result is that the differences between the two Catalogues are only to be found in the third place of decimals of a second, and there is, in fact, an absolute accordance between the Greenwich Catalogue of 1850 and the Cape Catalogue of 1850. I should be sorry to express a rash opinion upon the subject; but it is very suggestive that there is an agreement between the Greenwich observations and the Cape observations in 1850, when the reversible instrument was employed, and there is a discordance between the Cape observations in 1880 and the Greenwich observations in 1850, when non-reversible instruments were employed, and also between the Cape Catalogue of 1860 and the Greenwich observations of the same date. We know that the Greenwich observations do not differ very much from the normal observations of other observatories, and it is probable that the fault will lie in the Cape transit-circle, which is not a reversible instrument. We are now carrying out a series of observations to test it, observing stars reflex and direct in right ascension in pairs, and I hope I shall be provided with the means of erecting collimators at different altitudes to test the matter more directly. It is a question of peculiar interest in view of the question Mr. Common has brought forward, for it raises the question whether it is safe to work with non-reversible instruments; it is a mere accident of perfect homogeneity of the metal of which the instrument is made and absolute symmetry of construction if any instrument preserves its plan of collimation absolutely at all altitudes.

The next part of the work we have undertaken has been the reduction of a great number of occultations. Soon after my arrival at the Cape, I received from Professor Newcomb a letter requesting a series of Cape observations of occultations of stars by the Moon, and he informed me he was engaged in forming new tables of the Moon which were to be entirely founded on the results of occultations, and he was very anxious to obtain a series of occultations in the southern hemisphere. I replied that I should be happy to undertake the work; and Her Majesty's Government consented to provide the predictions of the occultations from the Nautical Almanac Office, so that the work has been carried out, and an immense number of such occultations have been obtained in the clear weather we have at the Cape. I was also happy to tell Professor Newcomb that there existed among the records of the Cape observatory between 400 and 500 occultations observed, a great number of which had not been published; but I may say that those made during Mr. Stone's period were all published in the Cape observations for all the years in which annual catalogues have been formed; but beyond this there exists a large amount of unpublished work, and not in a form very directly available for the computer; but Professor Newcomb undertook the computation of the places of the Moon for all observations before 1860, *i. e.* before Hansen's tables were introduced into the Nautical Almanac, and there is now in Her Majesty's Stationery Office an account of this work up to the end of 1880, with the equations in a completely reduced form compared with Hansen's tables of the Moon. That is work number 2, and that really refers to the amount of old work we have brought up, except the reduction of a great mass of extra meridian observations which is in a very forward state; but I have not thought fit to swamp Her Majesty's Stationery Office with it for immediate publication, though it will follow very soon.

I turn now from the old work to say something of the new. The meridian observations from 1879 to 1881 are in the press ready for publication. They contain observations at the rate of 5000 per annum, chiefly stars which were observed in connection with, first, the longitude, secondly with the determination of fundamental stars for the future observations of the stars of Schoenfeld's 'Durchmusterung,' and, thirdly, of stars observed with the equatoreal as comet comparison stars, and stars observed with the heliometer for parallax, and their corresponding comparison stars. A MS. also which I have brought with me contains a detailed account of the determination of the longitude of the Cape, of which an abstract has already appeared in the 'Monthly Notices.' The work which will most interest the Society, and on which I should like to say a few words, is the researches in which we have been engaged with regard to stellar parallax. When I was in Strasbourg in 1879, before going to the Cape, I met a young student, Dr. Elkin, a pupil of Professor Winnecke; he was then engaged in writing his dissertation on the parallax of α Centauri, and he requested me to send him observations which might have been made at the Cape of that star; at the same time I told him that I had acquired by purchase Lord Lindsay's Heliometer, and intended to have it mounted equatoreally at the Cape, to carry on the work of investigation of stellar parallax. This seemed to fire his enthusiasm, and he expressed a great desire to go with me and share my work. I said I could offer him no position; and he replied that he did not want one. So I said, "If you will be my guest, and come and live with me and share my work, you will be most welcome;" he said he should be happy to do so; and he came to me so soon as he had taken his degree. So we have undertaken a certain amount of work together, and I should like to give a short account of it. First, as to the method of the work. We endeavour to select a pair of stars as nearly as possible in the major axis of the parallactic ellipse of the star, the parallax of which we wish to determine, in order to get the maximum displacement. We try to get the stars equally distant from the central star, but within a distance 2° of arc. Sometimes we cannot quite secure that these stars are exactly opposite, and they may differ 10° or 15° from the straight line, but they should be nearly equal in distance and nearly equal in magnitude. We then place in front of the segment of the object-glass through which the brighter star is observed a screen of wire gauze consisting of one, two, three, or four folds according to the brightness of the star, till we have made a screen (sometimes of muslin, sometimes of wire gauze, and sometimes of a combination of both) which makes the three stars of precisely the same magnitude; and the field being slightly illuminated, the small diffraction spectra are totally obliterated. In the heliometer the star-disks are about 1″·0 in diameter, and there is little difficulty in causing one disk to pass through the other with an accuracy approaching one-tenth of the disk you see. At the meeting before last, when a brief account was given of this method, I think there was a doubt expressed that I should arrive at an accuracy of 0″·1 in measuring a distance of 6000″. Now there is no more difficulty in that than in measuring the space of 100″; and for this simple reason:—If, on account of an error in scale-value produced by temperature, or from some other cause, I measure the distance on one side too great, from the same cause I measure the distance on the other side too great; and if I deal with the difference of the two distances, and correct that difference on the assumption of the true distance between the comparison stars is a constant quantity, I eliminate any such changes in these differences; and therefore the measurement of two nearly equal opposite large distances can be accomplished with the same accuracy as a small distance, apart from the law of refraction, which, down to 70° of zenith-distance, is so well known that differential refraction is accompanied with an extremely small uncertainty. Therefore, as in the case of Sirius, where the stars of comparison are bright, we can measure these distances, not only with a probable error of 0″·1, but the *difference* of the two distances with a probable error of 0″·05. In the case of α Centauri we were anxious to arrive at a peculiarly accurate result; and not only did I take a pair of stars myself, but Dr. Elkin took another pair of stars at right angles to mine, and we got practically the same parallax within 0″·02. In the case of this star, we had to take an additional precaution. We have two stars, α Centauri being a double star; when we brought one of them to the brilliancy of a star of the seventh magnitude by means of a screen, the other remained inconveniently bright. Practically we found our errors were systematically affected by the neighbourhood of the fainter star, and the errors so produced were dependent upon the direction of measurement with the vertical, and we discussed those in this way. At the time when the stars culminate at midnight, it is possible to observe the star east of the meridian at any reasonable hour-angle, and also west of the meridian if you get up early in the morning, as we always did. Then you have simply to carry out a series of simultaneous observations east and west of the meridian, and you can at once ascertain whether your observations are affected by any systematic error. We traced these errors very carefully, and there were two suppositions on which we had to proceed. We might suppose the errors to depend (1st) on the direction of measurement relative to the line joining the observer's eyes; (2nd) on the direction of measurement relative to the vertical. The former hypothesis was found to represent the observations with great accuracy.

We also made another series of observations of α Centauri, in which very faint stars were selected, and the light of α Centauri was toned down by using screens so dense that the fainter component was practically obliterated, and then we could deal with the observations as if of single stars; and in that way we had four independent series of observations which practically agree in giving the same result of $0''\cdot75$ for the parallax of α Centauri. We proceeded in a similar manner with regard to Sirius, ε Indi, and other stars, and the results were given in the 'Observatory' and in the 'Register' two months ago. The only result not published was the parallax found for Canopus, which, despite the brightness of the star, is only small, viz. $0''\cdot03$, so that it is no nearer us than the faint comparison stars; the result is, that we seem to know little about the law which regulates the distances of the fixed stars. That leads me to a point I am very anxious to bring before the Society, and on which I desire to enlist its sympathy. I think it is a great work much required in sidereal astronomy. We ought to attempt to secure some information as to the distance of the fixed stars, their distribution in space, and the laws which regulate that distribution. In the memoirs which Dr. Elkin and I have presented to the Society there is a detailed plan, by which we propose, in the course of the next 10 years, to carry out such a work. I may say that Dr. Elkin has accepted the charge of the great heliometer at Yale College, and he is willing to cooperate with me, so that this work may be carried on in both hemispheres. The work will, as I have said, occupy about ten years, and I hope I shall be provided, by the liberality of the Government, with a proper heliometer for the purpose. It must be an instrument of 7 inches aperture, to deal with stars such as are necessary to get a sufficient selection; and for that purpose we shall have to observe about 16 stars in each hemisphere brighter than the second magnitude, 16 brighter than the third, and 16 brighter than the fourth, and 16 more of marked proper motion. That can be accomplished in less than ten years; and if we can get such an instrument, I am quite willing to give up my nights' rest to the work. Time presses; but I would like to say a word or two more as to the observations of Victoria and Sappho, which we called for. There has been an excellent response in the northern hemisphere; and having a large number of observations at my disposal, I will undertake the reduction as soon as I return to the Cape. But the pressure of work I have had in hand has rendered it impossible to undertake it before now. I may say the whole series of observations has not yet been received. There is another point in which the Society may take some interest. Soon after my arrival at the Cape, my attention was directed to the state of the Survey, and, thanks to Sir Bartle Frere, the late Sir George Colley, Sir Henry Mitchell, and Mr. Merriman, the present Minister of Public Works at the Cape, I have at last been able to get an annual budget raised, by which that Survey has been set on foot. Capt. Morris is in charge of the party, and Lieut. Laffan is with him with a staff of 14 men. I designed and ordered the instruments some two or three years ago, and they have duly arrived and are satisfactory. The base-line is finished and triangulation has begun; and to give you some idea of the accuracy of the work, the base has been measured in three sections, each section 1260 yards long, and each section measured twice. The greatest difference between any of the measurements was $\frac{4}{100}$ of an inch. Various tests have been applied, and there is little doubt that we have a correct base. It remains for us to determine the absolute lengths of the bars, and that is really the great difficulty. I have brought over the standard bar to England, and I hope to be able to compare it with the English standard and complete the thermo-coefficients of the bars at the Cape.

Tidal instruments have been mounted at Cape Town, East London, Port Elizabeth, and D'Urban, and I hope there will be regular tidal harmonic reductions. General Walker was good enough to lend me the great Theodolite which was originally constructed by Col. Strange for the Great Survey in India; but it was so long in coming that the triangulation was finished before it arrived; but it is a most desirable instrument for the purpose for which we want it, and I intend to devote the next two years to its use. The object is this: to the astronomer the question of refraction is an extremely difficult one, and I wish to devote it to the determination of absolute declinations independent of refraction, and I propose to observe with this instrument the azimuth of the greatest elongation of certain polar stars. That is all I have time to say with regard to my work at the Cape. It has been a busy time, and it is a great pleasure to come back and meet the Fellows of the Astronomical Society and tell them what I have been doing. I have been asked by Mr. Otto Struve to communicate an extract from a letter he has sent me bearing on this question of parallax. He states that during the publication of Volume x. of the Pulkowa Observations he has reduced a series of measurements made 30 years ago. From 20 observations of α Tauri he gets from observations of the position angle a parallax of $0''\cdot500$, with a probable error of $0''\cdot075$, and from measures of distance he gets $0''\cdot538$, with a probable error of $0''\cdot089$, and from the mean he gets $0''\cdot516$, with a probable error of $0''\cdot057$. The agreement of the values obtained by the totally different methods, distance and the position-angles, leaves no doubt as to the existence of a comparatively large parallax. The result is extremely interesting, and shows that there are still large parallaxes to be looked for among the stars.

Mr. Stone. As to the discrepancy between the Cape observations of 1850 and those for 1880, the amount does not appear to me so enormous as Mr. Gill thinks. It is not in excess of the amount of personalities in fixing absolute places, and an explanation may possibly depend to some extent upon systematic errors in the adopted clock-stars.

If such errors exist, and we know they do exist, the Cape observations about 1850 must agree with the Greenwich observations about 1850 near the equator, because the Cape clock errors depend upon the Greenwich adopted right ascensions. And the same will be the case with the Cape observations about 1880. But as the azimuth errors in all cases both north and south have been independently determined and the reference planes of the Transits have been made to pass through the respective poles, the existence of systematic errors will be shown by divergences between the equator and the poles. The Cape 1850 and Cape 1880 might therefore agree with the Greenwich observations 1850 and 1880 anywhere near the equator; but might differ between the equator and the poles. With respect to reversible transits, there are, no doubt, great advantages in their use, if properly used. If you use your instrument in one position for a length of time and get a fundamental determination, and if you reverse it and get the same result, you obtain a fairer approach to accuracy than with a non-reversible instrument. I should be glad to have at my disposal a good reversible instrument; but there is a great liability, when you reverse an instrument, to turn it into a new instrument. That was the view of Sir George Airy years ago.

Mr. H. Perigal. Why?

Mr. Stone. Because all the bearings and everything else are different when reversed. That was the key-note of Sir George Airy's objection to reversible instruments, and he would not introduce one. Whilst trying to get rid of one defect, you introduce another, and there is a great deal to be said for his view. I, for one, would never trust a reversible instrument except I could get an independent instrumental correction. If the results both ways agreed, I should have greater confidence in them; but if they disagreed, I should have to take the mean in the hope that the mean would be nearest the truth.

Mr. Downing. I should like to make a remark with regard to the question of right ascensions. In the catalogues published at the Cape and at Melbourne, and I believe in every catalogue published in the southern hemisphere, the right ascensions have been reduced by means of the Greenwich places of clock-stars; and that, I think, is objectionable, because there is a considerable difference between the equinox determined at Greenwich and at other places where the Sun is systematically observed; and it would be a great advantage to have an independent determination of the equinox at the Cape. That would give a greater value to any star catalogue that may hereafter be published; and I should like to point out how important it is that systematic observations of the Sun on the meridian should be made at the Cape. I should also like to express a hope that Mr. Gill will be able to give us a determination of the constant of aberration. We have lately had one from Pulkowa, which is probably as accurate as any that can be obtained in the northern hemisphere; but I should like to have it confirmed or corrected by observations in the southern hemisphere. There is no other place in the southern hemisphere besides the Cape where it is likely to be done, and I hope Mr. Gill will find the means of doing it.

The President. No one can have listened to Mr. Gill without being perfectly convinced that he has really undertaken a very important work at the Cape, and that he is doing it well. Ever since Mr. Gill was appointed H.M. Astronomer at the Cape, the eyes of English astronomers have been fixed upon him pretty strongly, particularly those of official astronomers; but I am certain he has borne our criticisms, and has come out of the ordeal in a remarkable manner. You have heard from him to-night of the immense work which he has undertaken, and which he has carried through with such success. I am sure few astronomers would have undertaken such a task, and have completed it in the successful manner Mr. Gill has done. Only a year or two ago he sent from the Cape his valuable paper on the solar parallax, determined from his observations in the Island of Ascension, which is published in our 'Memoirs.' The very sight of this paper when first delivered into our hands really astonished us, so enormous was the amount of work which it represented, the greater portion of which Mr. Gill must have prepared with his own hands. Then we have now, only a year or two afterwards, papers almost of the same size, although, for the convenience of printing, they have been cut down to as small a proportion as possible. I believe if Mr. Gill could have had his own desire these papers would have been double the size of those now in course of publication on that very important subject, the stellar parallax. I have no doubt that the investigations which form the subject of these papers are sensibly accurate, the probable error given being in each instance very small. Such being the case, I think we may congratulate Mr. Gill on having succeeded so well in such an important work. With regard to the discussion which has taken place with reference to the Greenwich Catalogue of 1850, it appears that Mr. Gill considers it important that the places from the Cape and Greenwich Catalogues of 1850 agree; but on this point we must bear in mind that the right ascensions of the Greenwich Catalogue of 1850 are of a composite nature, *i. e.* that one half of the right ascensions were observed with the old Troughton transit and the other half with the transit-circle, which came into use in January 1851.

Dr. Gill. Both good instruments.

The President. The observations for right ascension from 1848 to 1850 were made with the Troughton transit-instrument, and those from 1851 to 1853 with the transit-circle; therefore one half of the observations of the right ascensions were made with a reversible instrument and the other half with a non-reversible instrument; for this reason I consider the right ascensions of the 1850 Catalogue are very good, and I should say they would be less liable to systematic error in consequence of the employment of two instruments of different construction; for whatever systematic errors were due to the non-reversible instrument, they may be minimized by opposite systematic errors due to the reversible instrument. I mention this to show that the Greenwich 1850 Catalogue does not represent the non-reversed observations only. I have now only to thank Mr. Gill on your behalf for coming specially from Scotland to give us the interesting address which we have just heard. No one is more delighted than myself, as an old astronomer, to welcome Mr. Gill back again to England. (Applause.)

Mr. Knobel read a short note by *Mr. Marth* "On the determination of the planes of the orbits of Jupiter's Satellites." It was stated that the values of the inclinations of the orbits of the satellites were liable to a sensible amount of uncertainty. The eclipses of shortest duration furnished an indication of the value; but observations during successive eclipses still left considerable uncertainty in fixing the place of the nodes. So long as these observations were the only trustworthy means of finding the longitude and also the position of the orbits, this uncertainty had to be submitted to; but with the superior instruments now available advantage ought to be taken to employ them at proper times, to procure the elements which could be derived from eclipse observations with some degree of accuracy. The most promising means of determining the longitudes of the nodes would be by observing, by means of the micrometer, the distance at which the shadows of the satellites when making transits appear from the centre of a line bisecting Jupiter's disk perpendicularly to the axis of rotation. Though observers would not soon be favoured as they were in 1849 and 1860, when the opportunities were neglected, yet observers with superior telescopes and micrometers would be able to work at this matter during the opposition at the latter part of the present and the first part of next year.

Lieut.-Col. Tupman read a paper by *Professor Newcomb* respecting Mr. Stone's theory of the mean Solar day. Prof. Newcomb, without desiring to take up any grounds against Mr. Stone's conclusions, pointed out that Mr. Stone had not given sufficient information to enable him to submit the theory to a practical test. Prof. Newcomb also desired some further information as to the general definition of the epoch when the change took place.

Mr. Stone. I must confess I do not understand Professor Newcomb's position with respect to this question of the use of a variable unit of time in practical astronomy. Professor Newcomb says that the question is an elementary one. Surely, if that be the case, the question as to whether I am right or wrong can be settled without any such appeal to observations. But, finding my theoretical views disputed, I have appealed to the facts of observations, in the most direct manner possible, in a paper which cost much time and labour in its preparation. I brought before the Society, at the November meeting, a comparison between the observations made on the meridian of Greenwich with Hansen's Lunar tables, without any allowance for the errors of tabular mean time since 1864, and when the tabular places were corrected for such errors in accordance with my theory. The comparison, which extends over a complete revolution of the Moon's nodes, afforded the most positive evidence of the correctness of my views.

When respect to Professor Newcomb not understanding when the change dates from, I thought I had stated my point with a moderate amount of clearness. It is evident that the change of time begins at the instant when the changes in connection between the sidereal times and mean times were made. With respect to my explanation of the increase of errors in Hansen's tables, I am perfectly confident that I am right. I have merely applied to Le Verrier's expression corrections which make it absolutely identical with Bessel's day by day, and this is the only way that estimations of the effects of a change from Bessel's to Le Verrier's expression can be made.

The following papers were also announced:—

Maxwell Hall. "Variation in the Light of Neptune."

Dr. N. de Konkoly. "Spectroscopic Observations of the Red-coloured Sky at Sunset."

Dr. N. de Konkoly. "Spectroscopic Observations of Comet Pons-Brooks."

Dr. N. de Konkoly. "Electric Illumination of Spectroscope Micrometer."

R. de Kövesligethy. "On a new Colorimeter, serving as a Spectral Photometer."

R. de Kövesligethy. "Some remarks on the Spectroscopic Determination of the Motions of Stars in the Line of Sight."

Prof. C. V. Zenger. "A General Law of Planetary and Cometary Motion in the Solar System."

G. Davidson. "Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Davidson Observatory, San Francisco, and at Table Mount Station, California."

Dr. N. de Konkoly. "Remarks on Mr. Hilger's Illumination of Micrometers by Vacuum-tubes."

Rev. S. J. Perry. "Observations of Sun-spot Spectra in 1883."

Rev. S. J. Perry. "Phenomena of Jupiter's Satellites observed at Stonyhurst in 1883."

Capt. W. L. Rosseter. "Sextant Observations of Pons's Comet made at the U.S. Naval Observatory, Washington."

N. R. Pogson. "The New Comet of 1884, Jan. 12."

A. V. Nursing Row. "Observations of Comet *b* 1883 (Pons-Brooks) made at Vizagapatam."

E. J. Stone. "On the necessary Distinction in Practical Astronomy between the true Mean Solar day and the Mean Day adopted from time to time in the Construction of our Astronomical Tables and in the Comparison of these Tables with Observations."

Rev. S. S. O. Morris. "Sextant Observations of Comet α 1883 (Pons-Brooks) made at Monte Video."

Capt. W. Noble. "Occultations observed at Forest Lodge, Maresfield."

E. J. Spitta. "Note on the Transit of the 4th Satellite of Jupiter, 1884, March 12."

M. Nyrén. "Remarques sur les 'Notes on Nyrén's determination of the constant of Aberration' de M. Gill."

Communicated by *The Astronomer Royal.* "Observations of the Occultation of Venus by the Moon, made at the Royal Observatory, Greenwich, 1884, February 29."

Mr. J. T. Stevenson was duly elected a Fellow of the Society.

The Meeting adjourned at $10^h\ 10^m$ P.M.

Meeting of the Liverpool Astronomical Society.

The sixth Meeting of the Session was held on Monday, 17 March. The Rev. T. E. Espin, B.A., presiding. A paper by Mr. Joseph Baxendell, F.R.A.S., was read on the variable star R Arietis. After the publication of his early observations of this variable, he had discovered that one at least of the companion stars was slightly variable, and that in consequence some of the observations had been affected by errors which would have a sufficient influence upon the form of the light-curve to affect sensibly the concluded times of maxima. He had therefore made a redetermination of the times of the companion stars and a fresh reduction of the observations; they were:—

Maxima.

1859, Jan. 2.	1865, Feb. 18.
July 7.	1866, Feb. 21.
1860, Jan. 12.	Aug. 24.
1861, Jan. 12.	1867, Mar. 2.
1862, Jan. 24.	Sept. 5.
1863, Feb. 11.	1878, Dec. 6.
1864, Feb. 14.	1879, Dec. 19.
Aug. 18.	1880, Dec. 29.

Minima.

1859, Oct. 19.	1866, Dec. 4.
1861, Nov. 3.	1878, Sept. 14.
1862, Nov. 3.	1879, Sept. 20.
1863, Nov. 12.	1880, Sept. 24.
1865, Dec. 1.	1881, Oct. 2.

The equations formed from the dates of maxima, treated by the method of least squares, give:—

Mean period = 186·71 days.
Epoch = 1866, Sept. 1·3.

The minima, treated similarly, give:—

Mean period = 186·63 days.
Epoch = 1870, Jan. 2·3.

The mean interval from minimum to maximum is 87·7 days, and from maximum to minimum 99·0 days. This variable, therefore, belongs to the class in which the increase of brightness is more rapid than the decrease.

Adopting a mean period of 186·67 days, and calculating the times of maxima and minima, we have the following differences from the observed times:—

Maxima.

C–O.	C–O.
−1·75	−0·23
−1·04	+5·19
−3·33	+7·90
+4·09	+4·61
+0·51	+4·32
−9·07	+1·94
−3·65	−2·64
−2·94	−4·22

Minima.

C–O.	C–O.
−4·70	+5·12
−4·18	−4·39
+4·08	−2·13
+3·34	+1·13
−0·14	+1·39

The magnitude at maximum varies from 7·7 to 8·5, and at minimum from 12·7 to 13·5. Its colour, which is Smyth's orange No. 3, is also subject to changes which appear to follow the course of its variations of brightness, being deeper or more intense when the brightness is at a minimum, and lighter at or near a maximum.

Observations of the variable star R Leonis were contributed by Mr. J. E. Gore, F.R.A.S. The period was 312 days, but the elements showed a great irregularity, though no alteration seemed to have taken place.

In a review of the sun-spots of 1882–1883, Miss E. Brown gave a concise history of their formation and appearance during that time. Their maximum had now passed; therefore, unless there should be anything abnormal, we must be prepared for a cessation of those surprising outbursts which seemed, indeed, to have culminated about the end of 1882.

Captain W. Noble, F.R.A.S., described a singularly black appearance of the 4th satellite of Jupiter during its transit on the night of March 12.

In a paper on the Taurids and Muscids, Mr. W. F. Denning, F.R.A.S., said:—"These contemporary systems presented difficulties in both their positions and durations. They both seemed to hover about a certain general area, without maintaining an exact and well-defined centre of divergence. In fact, the visible behaviour of these streams, during the months of October and November, was so inconstant and erratic as to give the impression that a considerable number of concentric meteor-orbits were grouped together in the regions of Taurus and Musca."

The Rev. T. Perkins, M.A., gave an account of some recent experiments in lunar photography. His plan was to remove the eye-piece of his $8\frac{1}{2}$-inch Calver reflector, and substitute a plain brass tube. The sensitive plate was received by a piece of mahogany at the free end of the tube, and supported by two small screws. He had found an exposure of one second quite long enough.

Mr. W. J. Kidd thought photography was a very necessary adjunct to selenography. Many of the appearances were transient, and it would be as well if their occurrence could be fixed beyond dispute. He had noticed some formations obscure, whilst the neighbouring regions were well defined. This might be the result of mist, but some of them might also be caused by the dust arising from landslips, of which there were some examples.

A determination of the orbit of the binary star Castor was contributed by Mr. R. Wilding; and a brief reply to Prof. Smyth on the colours of 95 Herculis, by Mr. S. M. B. Gemmill.

Mr. Espin said the work of photographing star-charts was progressing favourably. In some of the pictures there were stars down to the eleventh magnitude. Several plates were passed round for examination, including one of the cluster near α Persei, in which upwards of 200 stars could be distinguished, and another of the Præsepe, showing the planet Mars.

The Secretary announced that the Rev. S. J. Perry, F.R.S. &c., would deliver an address on 21st April, on "Sun-spots; their Birth and Changes."

The Meeting adjourned at $9^h\ 30^m$.

The Present State of the Solar Activity.

The question as to whether the sun-spot maximum occurred in 1882 or is yet future is attracting considerable attention at present, and has been answered in very different senses by different astronomers. Thus the 'Comptes Rendus' of the Académie des Sciences for 1884, January 28 and February 11, contains the views of three high authorities on solar questions, and each presents a different conclusion. M. Wolf considers that "up to the present time we have not the means for determining with certainty the moment when the phenomenon has passed or will pass the maximum," for, as he points out, considerably the highest *monthly* maximum took place in 1882 April, the time when he had expected the period of maximum activity to set in; and yet, on the other hand, the relative spot number for the *year* 1883 is slightly but unmistakably higher than that for 1882. M. Faye on his part considers that the maximum is undoubtedly past; for if half-yearly instead of yearly means be taken, the first half of 1882 shows a higher number than any of the three following half-year periods. The great outburst in 1882 April, therefore, in his opinion, was the true epoch of maximum, the outbursts of 1882 November, 1883 April, 1883 June–July, and 1883 October–November being just such secondary maxima as "might well occur in the progress of a periodic phenomenon which passes rapidly and without hesitation from a minimum to the following maximum, but which passes gently by a series of secondary oscillations from the maximum to the following minimum," as it is well known the solar spots do. M. Faye is further confirmed in his view by M. Wolf's statement that there were no days without spots in 1882, and four in 1883, and he remarks that Schwabe discovered the periodicity of the spots, not by counting them, but by registering the number of days when the Sun was free from them. The increase in the number of days without spots should on this view be clear proof that the maximum is already past.

M. Tacchini, on the other hand, considers that the solar activity has increased during 1883; "for although the difference in the number of spots is very small, the number of groups in 1883 has been very much greater, and the extent of the spots has been truly extraordinary; it has been double that of 1882."

Yet another order of facts are brought to bear on the question by Prof. Spoerer, who in a paper on this subject in the 'Astronomische Nachrichten,' No. 2565, gives some interesting deductions from the numbers and positions of the spots observed during the last 30 years. Dividing the interval from minimum to minimum into ten periods, and arranging the spots according to their heliographic latitude in zones each 5° in breadth, he exhibits in a table the mean numbers observed for each zone in each period. The table brings into prominence a remarkable feature of the behaviour of sun-spots, to which Prof. Spoerer and other observers have called attention before, but which has hardly yet received the recognition it deserves. It is well known that on the whole spots are most numerous about lat. 17° or 18°; and that by far the greater number lie between lats. 25° and 10°; but this table brings again into prominence a fact less widely appreciated, viz. that from the time of one minimum until the next, the region of greatest spot frequency gradually drifts downward from the zone 30°–25° to the immediate neighbourhood of the equator, and that about the time of maximum its seat lies about 17° or 18°. As the next minimum period is approached, spots more than 15° from the equator gradually become rarer than spots of 35° lat. and upwards were at the time of maximum. But directly the time of minimum is past, spots begin to appear again in those higher latitudes where but very few, perhaps not a single one, had been seen for several years. This sudden transference of the display of energy from a zone where it has been manifested without interruption year after year to another zone, and that the most distant, from which any such display has long been absent, is one of the most striking facts which solar research has revealed to us. No theory of the cause of the periodicity of sun-spots can be deemed satisfactory which does not fully recognize this remarkable fact—a fact which seems inconsistent with the theory which ascribes the sun-spot period chiefly to planetary influence.

An examination of Prof. Spoerer's table shows, as he points out, that as to spot distribution in latitude there was a greater resemblance to the period 1882·53–1883·65, in the periods immediately succeeding the maxima of 1860 and 1870, than to the periods of actual maxima themselves. This would seem to show that the decline had already begun, the richest zone being not that extending from 15° to 20° as usual at the maximum, but that from 10° to 15°, which ordinarily shows fewer spots than the outer zone until the spots have begun to diminish both in number and area.

It is clear from the statement of these four different opinions that much of the difficulty in arriving at a conclusion as to whether the maximum is past or not arises from our inability to determine which of the various data before us is to be accepted as affording the best index of the magnitude of the solar activity. Perhaps it would be most reasonable to consider that the areas of the spots gave the truest indication, rather than their numbers, or the percentage of days on which they were seen, since a single large spot may have an area equal to the sum of a score of small ones, and it is noteworthy that the great magnetic storms have coincided, not with the display of unusual numbers of small spots on the Sun's disk, but with the appearance or rapid development of some single spot or group of spots of abnormal extent. M. Tacchini's statement, "l'extension des taches a été vraiment extraordinaire; elle a été double de celle de 1882," would seem to put it beyond dispute that the maximum was still future in 1882. Each year from the time of minimum has shown a mean daily spotted area about double that of the preceding. If 1883 had shown the same proportion it would have proved that we were still ascending the steep face of the curve. M. Tacchini's figures appear to show that the preponderance of 1883 over 1882 has been even greater than this, his results being as follows:—

Daily Means.	1882.	1883.
Of the Number of Spots and Pores . .	22·57	20·14
Of the Number of Groups	4·37	4·99
Of the Area of Spots	59·19	125·54
Of the Area of Faculæ	81·54	89·44

Unfortunately, the series of observations from whence these numbers are derived is not a continuous one; the numbers therefore cannot be considered as exactly representing the true daily means for either year; and the results obtained from the measurements of the photographs taken at Greenwich and at Dehra Dun show that the mean daily area for 1883, as compared with that for 1882, was much smaller than M. Tacchini states. Still the photographs show a decided increase in the spotted area in 1883, though by no means in the same proportion as in former years, and therefore to this extent they confirm M. Tacchini's opinion that the maximum was scarcely attained at the close of that year. There seems also to have been a decided tendency in the spots to seek a higher latitude towards the end of 1883; so that had Prof. Spoerer's paper been continued so as to embrace the spots up to the end of the year, instead of terminating with those of August, his conclusions might have been somewhat modified. Since also both the numbers of spots and of spot-groups would seem to have slightly increased, four different modes of estimating the magnitude of the solar activity concur in representing an increase, but only a slight one, to have taken place in 1883 as compared with 1882. On the other hand, there have certainly been more days in 1883 when the Sun's disk was free from spots than in the year preceding. But when it is borne in mind that the spots have never been equally distributed over the Sun's disk, and that during the five years' progress upward from the minimum they have abounded in certain longitudes and been very rare in others, so that the entire period has shown a succession of oscillations the mean period of which has been nearly that of the Sun's synodic rotation, it will appear that the entire absence of spots from the Sun's disk for a single day scarcely means more than a slight exaggeration of a fact which every rotation has made obvious, viz. that one hemisphere of the Sun has been much less prolific in spots than the other. Succeeding days have given clear proof that though one side of the Sun showed no spots, the other side was by no means free from them. The fact that a single day now and then has shown a spotless Sun should, so long as other indications seem to point toward an increase in the solar energy, be regarded rather as accidental than as proving that the period of decline has commenced. It should, however, be remembered that no single day has yet shown us so great a spotted area as that seen on 1882 April 21, or so large a single spot as that of 1882, November 12–25. If, then, we fixed the maximum with regard to the greatest single display instead of by the greatest average display, one of these dates would seem to have the greatest claim, though our election might in that case be overthrown as soon as made by the appearance of some fresh spot or spots, vaster even than those alluded to. The fact is rather that whilst after the minimum there is a period of about three years in which the spots increase continually and with considerable rapidity, and after a maximum a somewhat longer period in which they diminish, there is at both minimum and maximum, but especially at the latter, a period of many months in which it is scarcely correct to say that they either increase or diminish. In short, the minimum and maximum are periods, not points, of time.

Observations of Comet Pons-Brooks, 1883–4.

In connection with other astronomical work, I have on several occasions lately, while the comet was approaching perihelion, given some attention to observations of the tail as regards its approximate length and the apparent brightness of the nucleus and coma combined, as compared with the fixed stars. I have used in making these observations an opera-glass magnifying $2\frac{1}{2}$ diameters, and a field-glass magnifying 4 diameters. Occasional observations have also been made with my Clacey equatoreal of $4\frac{3}{8}$-in. aperture. The moonlight and cloudy weather have interfered with the observations to a great extent, but the few details gathered are considered of sufficient value to place upon record. Any one who has given attention to the matter of comparing the brightness of comets with the fixed stars will recognize the difficulty of making such comparisons; and after repeated and unsatisfactory trials, I was led to observe both the comet and the comparison stars out of focus, expanding both to disks of light, this method also being used to some extent in observing variable stars.

By so doing, the light of both the nucleus and coma were included in the estimations of brightness; but this would not materially affect the results, and some approximation can be arrived at as to the apparent brightness of the comet at the present apparition. The first observation of the comet was made with my Clacey equatoreal on the evening of Dec. 21, at 8^h. With a power of 40 I could trace the tail about $2°$.

Jan. 3. Saw the comet very plainly with the naked eye at $5^h\ 20^m$. At $7^h\ 45^m$ could trace the tail in the field-glass $4\frac{1}{4}°$, and with the opera-glass $2\frac{1}{2}°$ (moonlight). The comet was 1 or 2 steps brighter than μ Pegasi, or about 4 mag. Looked for trace of secondary tail with the telescope, but without success.

Jan. 9, $6^h\ 30^m$. Moonlight and slightly hazy. Tail in field-glass $2\frac{3}{4}°$ long. Light 2 steps fainter than ζ Pegasi.

Jan. 16, 7^h. Tail in field-glass $11\frac{1}{4}°$ long, with naked eye $6\frac{1}{2}°$. Light 4 steps brighter than ζ Pegasi, 5 steps fainter than γ Pegasi, or about $3\frac{1}{2}$ mag. Southern edge of comet's tail the brightest and best defined. Several stars seen through the tail (one very near the nucleus) with undiminished brightness.

Jan. 17, 6^h. Tail in field-glass $11\frac{1}{2}°$ long; eye $6\frac{3}{4}°$. Brightness 4 steps $>$ ζ Pegasi, 5 steps $<$ γ Pegasi.

Jan. 18, $6^h\ 30^m$. Tail in field-glass $11°$ long; eye $6\frac{1}{4}°$. Brightness same as 17th.

Jan. 21, $6^h\ 30^m$. Length of tail in field-glass $10\frac{1}{4}°$; eye $5\frac{1}{4}°$. Light 3 or 4 steps $>\zeta$ Pegasi, 5 steps $<\gamma$ Pegasi.

Jan. 22, $6^h\ 30^m$. Hazy; apparently fainter.

Jan. 26, 7^h. Comet getting very low and rapidly diminishing in brightness. Light 4 or 5 steps $<\zeta$ Pegasi, $=\iota$ Ceti. Tail in field-glass $9\frac{1}{4}°$ long; opera-glass $6\frac{1}{2}°$; eye $3\frac{1}{2}°$.

Feb. 1, $6^h\ 30^m$. Moonlight. Light $=\alpha$ Sculptoris. Tail in field-glass $3\frac{1}{4}°$ long?; very low. EDWIN F. SAWYER.

Cambridgeport, 1884, Feb. 10.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Saturn.

SIR,—

You have given a description of a new dark belt on the disk of Saturn; as I have observed this planet in the year 1883, since Aug. 29, the following details will perhaps be interesting to your readers. This belt was noticed and sketched for the first time on Oct. 25; it was then easily seen. Cloudy skies prevented me from seeing it again before Nov. 13, for which day I find in my observing-book the following remarks:—"The shadow of Saturn on the rings is very remarkable to-day; on both sides of the disk two little angular shadows are seen; the shadow on the left side is larger than that on the right. The structure of the belts is also very interesting; at the equator of Saturn two dark belts present themselves, the lower nearly black, the upper one not so dark; the disk is greyish in colour from this latter belt to the south pole. At the pole itself a darker district is noticed. In the northern hemisphere, close to the ring C, the dark belt remarked on Oct. 25 is seen again; it seems to me to-day fainter than on the preceding occasion."

Since then this belt has been seen again repeatedly. Its curvature is slight, but at the same time unmistakable. I would add that, in consequence of the repair of the regulator, Saturn has not been observed between Oct. 7 and Oct. 27. At the earlier date the dark belt was not visible. The grey colour of the S. hemisphere was remarked from the first evening of observation, and also the peculiarity in the shadow of Saturn on the rings; this latter has always appeared to me under the form of a rounded angle.

These observations have been made with the Plantamour equatoreal, of 10 inches aperture.

Yours truly,

A. KAMMERMANN,
Assistant Astronomer.

Observatoire de Genève,
1884, Mar. 18.

spot near the limb, the sphericity of the Sun, and the height of the faculæ above the spot it was possible to look *under* the faculæ to the east of the spot, and to see much of the penumbra beneath it. "On this part of the penumbra an extraordinary appearance was perceived, which resembled so closely a shadow as it would have been cast by the overhanging faculous mass, that it seemed useless to seek, and it was impossible to admit, any other explanation than this, the matter appeared so plain, and so impressed the mind." This shadow, the outline of which was a little diffused, had the same shape, and reproduced with great exactness the outline of the faculous mass situated about it. It was not as black as the opening in spots called the *umbra*, but of a very dark tint, which, however, allowed the radiated structure of the penumbra it covered to be recognized. M. Trouvelot had made similar but less striking observations on other occasions, of which he mentions, 1872, Feb. 28; 1872, March 16; 1877, Oct. 28; and 1877, Nov. 4. From these observations M. Trouvelot infers that the bright light of the faculæ does not penetrate much below the surface; but is probably generated at or near it; and should this be a general occurrence, it appears not unlikely that the "brilliant light emitted by the faculæ, and perhaps all the solar light, is generated at the surface, the presence of the coronal atmosphere being perhaps essential to its production."

Saturn and Uranus*.—Under most exceptionally favourable atmospheric conditions the following interesting observations of Saturn on March 16, and Uranus on March 18, were made by Messrs. Thollon, Perrotin, and Lockyer with the 14-inch equatoreal of the Nice Observatory. The outer ring of Saturn appeared to be composed of three distinct rings, the innermost being the broadest, the outermost the narrowest, but the differences of breadth being only slight. Each of these rings when best seen showed striæ towards the ansæ which gave the impression that they consisted of numerous divisions. The division nearest the planet was seen for nearly the whole breadth of the ring, the distance from the inner edge of the ring being a little more than one third of the breadth of the ring and best seen in the west ansa (inverted image); the next division was best seen near to the east ansa, both appearing concentric with the ring.

The definition was exceedingly good at 10 P.M. on the 18th whilst Uranus was being examined. The general appearance of the planet was to some extent similar to that of Mars—dark spots similar to those on that planet being seen towards the centre of the disk, and at position-angle 380° a white spot was seen on the limb, recalling the Martial poles. Two different tints were perceived on the disk, the colour of the N.W. hemisphere being dark and that of the S.E. a bluish-white colour, the same appearances being seen when the instrument was rotated through 180°.

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* Comptes Rendus, Vol. xcviii. No. 12.

an undefined central condensation. We are waiting another good observation for a third place before computing its orbit. It must have passed its perihelion about Jan. 12th; it is now moving slowly north and east.

Positions.

	Melb. M.T. h m s	R.A. h m s	Decl. ° ′ ″
Jan 12......	9 3 29	22 3 37·0	−40 7 14 S.
18......	8 31 25	22 49 54·5	−41 40 22
28......	10 41 30	23 33 45	−41 55 54
29......	9 29 3	23 36 53	−41 54 10

The *Pons* Comet is now waning, but still well seen with the naked eye on dark nights. Nucleus sharp and stellar.

Position on Jan. 30th, at $8^h\ 25^m$, Melbourne mean time, was $0^h\ 25^m\ 21^s{\cdot}7$, $-25°\ 15'\ 27''{\cdot}2$ South.

Observatory, Melbourne,
1884, Feb. 1.

Yours faithfully,
R. L. J. Ellery.

Sir,—

In the 'Echo' and the 'Evening News,' Sydney newspapers, of the 4th and 5th instant respectively, there appeared a notice, copied from the 'Launceston Examiner,' to the effect that a bright comet had been seen at New Norfolk, Tasmania, at 4 o'clock, A.M. on the 27th December. It was described as being a few degrees above the horizon and bearing about east. As soon as the weather permitted I made a search in the morning sky for this object, but without success. It was, however, quite possible that the comet might have passed conjunction with the Sun, and might soon make its appearance in the evening sky. On the evening of the 14th I received from the Melbourne Observatory a telegram to the effect that a small comet had been found there, and giving its position and daily motion for the 12th. Owing, however, to the cloudy and smoky state of the atmosphere I was unable to find the comet till the evening of the 19th. It appeared then to be just beyond the reach of unassisted vision. In the telescope it appeared as a round nebula about two minutes of arc in diameter, gradually condensed towards the centre. There was no definite nucleus in the micrometer eyepiece, but a short tail was perceptible. The comet was well observed on the 19th, 21st, 22nd, 23rd, 24th, 25th, and 27th ; it faded rapidly and is now observed with difficulty. Whether this comet is identical with that seen in Tasmania it will be impossible to say till the orbit is computed. It is quite possible that it has escaped observation in the northern hemisphere, and I therefore avail myself of the mail which is now leaving for Europe to send you the results of my measures on the first four evenings. A square bar-micrometer was employed on the $4\frac{1}{2}$-inch telescope. The measures are not yet completely reduced, but they may be relied on to a second or two of arc in either co-ordinate. My time is so fully occupied that I am unable to send the results in a more complete form.

It will be seen that excellent authorities are available for the places of the comparison stars.

Windsor Mean Time, 1884.	Comet—Star. Diff. R.A.	Comet—Star. Diff. N.P.D.	No. of Comps.	Comp. Star.
d h m s	m s	′ ″		
January 19 9 37 42	+5 19·33	+ 3 55·6	6	*a*
„ 19 9 37 42	+0 45·63	+ 0 33·9	6	*b*
„ 21 9 26 7	−3 28·76	+10 51·6	4	*c*
„ 21 9 31 39	−5 21·15	+ 5 15·5	5	*d*
„ 22 9 3 4	+1 13·66	+13 13·0	8	*c*
„ 22 9 3 4	−0 40·12	+ 7 31·6	8	*d*
„ 23 9 22 11	+5 47·57	+14 37·7	5	*c*
„ 23 9 22 11	+3 53·75	+ 8 56·4	5	*d*

The comparison stars are identified as follows:—

a=Cape Cat. 1880, No. 11941.

b=Wash. Cat. 1860, No. 10109, and Cape Cat. 1880, No. 11966.

c=Wash. Cat. 1860, No. 10232; Melb. Cat. 1870, No. 1183; and Cape Cat. 1880, No. 12062.

d=Wash. Cat. 1860, No. 10249, and Cape Cat. 1880, No. 12080.

Observations of Pons's Comet were commenced on the 13th.

Windsor, N. S. Wales.
1884, Jan. 28.

Yours faithfully,
JOHN TEBBUTT.

NOTES.

PROF. KLINKERFUES.—Ernst Friedrich Wilhelm Klinkerfues was born at Hofgeismar, in Hesse, on the 29th of March, 1827. His father held a civil appointment there, the salary of which was very small, involving him in many of the troubles incident to straitened circumstances. Young Klinkerfues made his first studies at the Polytechnic School at Cassel, and afterwards obtained a situation as surveyor. At Marburg he was brought into contact with Prof. Gerling, who, recognizing his talents, gave him some instruction in theoretical and practical astronomy, and then advised him to repair to Göttingen, after sending a very strong recommendation to Gauss to employ him at the observatory. This was in 1851, Klinkerfues having already mastered the 'Theoria Motûs.' After making trial of his abilities and perseverance, Gauss accordingly procured him the post of assistant at the Göttingen observatory. Four years afterwards Gauss died, and Klinkerfues was nominated "Observator," obtaining the same year (1855) his doctor's degree by writing a "Dissertation on a General Method of calculating the orbits of Double Stars." In 1863 he was nominated Extraordinary Professor in the Faculty of Philosophy, and in 1868 Director of the Observatory in the section of Practical Astronomy. As early as 1849 he had begun to publish papers in the 'Astronomische Nachrichten,' communicating on different occasions observations of a lunar eclipse, of Neptune, of occultations, of comets and small planets, of the magnetic variation during appearances of auroræ, &c. He discovered Comets III. 1853, I. 1854, III. 1854, IV. 1854, II. 1855, and V. 1857. He observed the solar eclipse of the 18th July, 1860, at Cullera in Spain. He calculated the elements of several comets and small planets, and the orbits of two double stars (ω Leonis and p Ophiuchi). His theoretical labours are also of importance, in regard to the connection between comets and meteoric streams, and the investigation of the influence of the motion of a source of light upon the refrangibility of its rays. His work on 'Theoretische Astronomie' is well known; and his academical lectures embraced both spherical and theoretical astronomy and the theory of perturbations. Between the years 1855 and 1863 he carried out a large number of zone observations of fixed stars, in conjunction with Prof. Schering, which still await publication. Prof. Klinkerfues was elected an Associate of our Astronomical Society in 1882. It is much to be regretted that the straitened means and sad circumstances of his youth cast a lasting gloom upon a mind so richly endowed; this he was never able entirely to shake off, and it probably contributed to his melancholy end, which occurred on the 28th of January last at the Observatory.

Dr. Julius Schmidt.—Johann Friedrich Julius Schmidt was born at Entin on the 26th of October, 1825. From early youth he manifested an enthusiastic taste for the independent study of natural phenomena, especially in the department of astronomy; and in following out this inclination he was much aided by the possession of an eyesight keenly sensitive to variations of form and gradations of light and colour. After he had completed his course of study at the gymnasium at Hamburg, he devoted himself to astronomical research, giving the greatest part of his attention to the physical appearances of the heavenly bodies. In 1845 he went from Hamburg to Bilk, near Düsseldorf, to assist Prof. Benzenberg, who had set up an observatory there for the purpose of observing shooting-stars and searching for intra-Mercurial planets. Benzenberg died the year after, upon which Schmidt accepted the situation of assistant to Argelander at the new observatory at Bonn. There he laboured for seven years at the regular course of observations of planets and comets, taking part also in those of stars for Hour V. of the Berlin Academy star-charts, which were afterwards published. Whilst working at Bonn he went to Rastenburg, in East Prussia, in 1851, to observe the memorable eclipse of the Sun which occurred there that year. He left Bonn in 1853 to undertake, at Argelander's recommendation, the charge of the Baron von Unkrechtsberg's private observatory at Olmütz, in Moravia, whence he removed to Athens in 1858, being appointed Director of the Observatory there, to the duties of which he devoted himself with great assiduity during the remainder of his life. His papers and observations on the changes of light of the variable stars, the solar spots, the markings on the surfaces of the planets and the times of their rotation, nebulæ and meteors, and especially the physical appearances of comets, are well known to all who have followed the records of astronomy. Nor need we speak here of his noble map of the Moon, which was published at last in 1878 at the expense of the German Government. But it should not be forgotten with what comparatively small instrumental means Dr. Schmidt's careful and persevering labours were carried on, as this enhances the merit of the long continuance of many of his observations, favoured as they were by the fine sky and southern position of Athens. These, however, did not exhaust the whole of his energy, for he made besides many observations of volcanic phenomena and contributions to our knowledge of the physical geography of Greece. Dr. Schmidt was admitted an Honorary Doctor by the University of Bonn, on the occasion of its foundation-festival held in 1868, and was elected an Associate of our Astronomical Society in 1874. It is a noteworthy circumstance that the same number (2577) of the 'Astronomische Nachrichten' which contains his obituary has also a paper giving his last series of observations of variable stars made during the year 1883. His death occurred very suddenly from disease of the heart, on the night of the 6th of February last. He was found dead in his bed on the morning of Thursday the 7th, after having been apparently in perfect health the previous evening, which he had spent at the German embassy at Athens. The occasion of his funeral was fitly made one of national mourning in that capital.

[For these obituary notices, we are mainly indebted to those in Astr. Nachr. Nos. 2573 and 2577.—W. T. L.]

WASHINGTON NAVAL OBSERVATORY.—Rear-Admiral Shufeldt, who was appointed director of the Washington Naval Observatory, in place of Vice-Admiral Rowan, on May 1, 1883, has recently presented the annual report to the Chief of the Bureau of Navigation. From this report it appears that the transit circle, meridian-transit instrument, and 9·6-in. equatoreal have been actively employed in the same class of work as in former years. Observations with the PrimeVertical instrument were re-commenced on 1882, Nov. 14, and embraced continuous observations of about 40 stars (within 2° of the zenith at meridian passage), at the time of the two maxima of aberration, except in the case of α Lyræ, which has been regularly observed throughout the 24 hours, having in view the possibility of a determination of its absolute parallax. About 580 observations have been obtained, the reduction being performed by

Struve's formulæ. The 26-in. equatoreal has been employed for observations of double stars, satellites, and comets. A change in the raising and lowering of the canvas covering for the opening in the dome, in order to avoid the friction of the wire ropes, has worked satisfactorily. Saturn's ring has been observed, but many of the strange phenomena noted by other observers have not been noted even on the best nights. During the greatest opening of the ring, micrometric measures of its dimensions will be taken. Some measures for stellar parallax were undertaken, but it is considered advisable to defer this work until more convenient arrangements are made for housing the observer. The satellites of Saturn, Uranus, and Neptune have been observed. A large number of these observations have been collected, and their discussion has been undertaken in order to correct the orbits of the satellites and to determine the masses of the planets. The meteorological observations have been taken by the watchmen as in former years. The report also gives an account of the progress made in the reduction of Gilliss's zone observations 1850–51 and 52, the chronometer department, and the nautical instruments. On Feb. 21 Admiral Shufeldt was placed on the retired list, Commodore S. R. Franklin, of the Naval Examining Board, being appointed Director.

Maxima and Minima of Variable Stars in 1884, *April.*

April	1	T Hydræ, M.	April	13·9	W Virginis, *m*.
	4	U Monocerotis, *m*.		18	S Coronæ, M.
	4·8	W Virginis, M.		22·1	W Virginis, M.
	5·6	T Monocerotis, *m*.		24	V Virginis, M.
	6	R Lyræ, M.		24	R Aurigæ, M.
	11	R Virginis, M.		26	χ Cygni, *m*.
	13·6	T Monocerotis, M.		26·5	β Lyræ, *m*.
	13·6	β Lyræ, *m*.		28	U Monocerotis, M.

M, signifies maximum ; *m*, minimum.

Variables of Short Period.

δ Cephei.	ζ Geminorum.	Algol, *m*. h m s	U Cephei, *m*. h m s
Apr. 2·15, *m*.	Apr. 2·7, M.	Apr. 5 14 12	Apr. 4 9 12
3·75, M.	7·8, *m*.	8 11 1	9 8 52
7·55, *m*.	12·8, M.	11 7 50	14 8 31
9·15, M.	18·0, *m*.	25 15 54	19 8 11
12·95, *m*.	23·0, M.	28 12 43	24 7 50
14·55, M.	28·1, *m*.		29 7 30
18·25, *m*.	S Cancri, *m*. h m	δ Libræ, *m*. h m s	U Coronæ, *m*. h m s
19·85, M.	Apr. 13 16 36	Apr. 7 13 48	Apr. 3 12 50
23·65, *m*.		14 13 22	10 10 33
25·25, M.		21 12 56	17 8 15
29·05, *m*.		28 12 30	27 16 49
30·65, M.			

Approximate Times of Transit of the great Red Spot and Equatoreal White Spot across the Central Meridian of Jupiter in April and May 1884.

Red Spot.				White Spot.			
Day of Month.	On C.M.	Day of Month.	On C.M.	Day of Month.	On C.M.	Day of Month.	On C.M.
	h		h		h		h
Apr. 1.	9·8	Apr. 25.	9·7	Apr. 1.	14·2	Apr. 25.	8·7
3.	11·4	27.	11·4	2.	9·9	27.	10·0
4.	7·3	30.	8·9	4.	11·1	29.	11·2
5.	13·1	May 2.	10·5	6.	12·3	May 2.	8·1
6.	9·0	5.	8·0	7.	8·0	4.	9·2
8.	10·6	7.	9·7	8.	13·4	6.	10·4
10.	12·3	9.	11·3	9.	9·1	8.	11·6
11.	8·1	12.	8·8	11.	10·3	11.	8·4
13.	9·8	14.	10·5	13.	11·5	13.	9·6
15.	11·4	17.	8·0	15.	12·6	15.	10·8
17.	13·1	19.	9·6	16.	8·3	20.	8·8
18.	8·9	24.	8·8	18.	9·5	22.	10·1
20.	10·6	26.	10·4	20.	10·7	27.	8·2
22.	12·2	29.	7·9	22.	11·9	29.	9·3
23.	8·1	31.	9·6	23.	7·6	31.	10·5

Further observations of the red spot have been obtained at Bristol as under:—

		On C.M.	Long. (870°·42).
1884, February	21......	6^h 46^m	85°·3
March	11......	7 28	88 ·5
"	15......	10 48	90 ·9

The equatoreal white spot has been observed as follows:—

		On C.M.	Long. (878°·46).
1884, February	19......	9^h 6^m	78°·7
"	23......	11 36	84 ·2
"	24......	7 9	79 ·8
"	26......	8 23	81 ·9
March	2......	6 21	80 ·4
"	11......	6 51	83 ·9
"	15......	9 15	85 ·9

Since the middle of February, the white spot has been very bright and well defined. It will come to conjunction with the red spot on April 21. The latter is still visible, though a difficult object, and will become more so, as the diameter of the planet is getting smaller and his position less favourable.

Bristol, 1884, March 17. W. F. Denning.

Astronomical Memoranda, 1884, *April*.

Equation of Time:—Sun *after* Clock, April 1, $3^m\ 46^s$; April 11, $0^m\ 54^s$: *before* Clock, April 21, $1^m\ 28^s$; April 30, $2^m\ 58^s$.

Sidereal Time at Mean Noon:—April 1, $0^h\ 41^m{\cdot}0$; April 11, $1^h\ 20^m\ 25^s$; April 21, $1^h\ 59^m\ 51^s$; April 30, $2^h\ 35^m\ 20^s$.

Sun.	Rises.		Sets.		Position-angle of axis.		Heliogr. co-ordinates of centre of disk. Lat.		Long.	
	h	m	h	m	°	′	°	′	°	′
April 1	17	34	6	31	333	35	−6	23	9	8
6	17	23	6	40	333	30	6	4	303	10
11	17	12	6	48	333	36	5	42	237	9
16	17	1	6	57	333	54	5	18	171	7
21	16	50	7	5	334	22	4	52	105	4
26	16	40	7	13	335	2	4	24	39	0
May 1	16	31	7	20	335	52	−3	53	332	56

The position-angle of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon.

Moon.	*sets.* h	m		*rises.* h	m		*rises.* h	m
April 1	13	1	April 11	8	7	April 21	15	14
2	13	50	12	9	8	22	15	42
3	14	29	13	10	6	23	16	11
4	15	5	14	11	1	24	16	43
5	15	31	15	11	50	25	*sets.*	
6	15	57	16	12	34	26	8	33
7	16	21	17	13	13	27	9	47
8	16	45	18	13	47	28	10	51
9	17	9	19	14	18	29	11	45
10	17	35	20	14	46	30	12	29

First Quarter, April 2, $9^h\ 17^m$; Full Moon, April 9, $23^h\ 44^m$; Last Quarter, April 18, $3^h\ 55^m$; New Moon, April 25, $2^h\ 58^m$.

Mercury is an evening star throughout the month; at greatest elongation (20° 21′ E.) April $25^d\ 2^h$, about which time it is very favourably situated for observation.

Venus, an evening star, at greatest Hel. Lat. N. April $24^d\ 19^h$, in conjunction with the Moon April $28^d\ 8^h$. Diameter:—April 1, 17″·4; April 30, 22″·8. Illuminated portion of disk, ·593 on April 15.

April 1, R.A. $3^h\ 29^m{\cdot}2$, Dec. 21° 0′ N., tr. $2^h\ 48^m$, sets $10^h\ 47^m$
30, 5 41 ·9, 26 41 N., 3 7 11 49

Mars is retrograding in Cancer, in conjunction with the Moon April $4^d\ 4^h$. Diameter:—April 1, 11″·1; April 30, 8″·8. Illuminated portion of disk ·904 on April 15.

April 1, R.A. $8^h\ 31^m{\cdot}1$, Dec. 21° 54′ N., tr. $7^h\ 49^m$, sets $15^h\ 54^m$
30, 9 7 ·3, 18 41 N., 6 31 14 16

Jupiter in conjunction with the Moon April $3^d\ 7^h$ and $30^d\ 18^h$. Diameter:—April 1, 37″·0; April 30, 33″·8.

April 1, R.A. $7^h\ 47^{m}\cdot1$, Dec. 21° 48′ N., tr. $7^h\ 5^m$, sets $15^h\ 10^m$
30, 7 56 ·8, 21 22 N., 5 20 13 22

Saturn in Taurus, is in conjunction with the Moon April $27^d\ 7^h$.

April 1, R.A. $4^h\ 18^{m}\cdot1$, Dec. 19° 49′ N., tr. $3^h\ 36^m$, sets $11^h\ 29^m$
30, 4 31 ·2, 20 24 N., 1 55 9 51

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
April 3	38″·93	17″·07	25″·89	11″·35	15″·4
23	38 ·03	16 ·83	25 ·29	11 ·19	15 ·0

The south side of the rings is visible, the elevation of the Earth above their plane being S. 26° 0′ on April 3, and S. 26° 16′ on April 23, and of the Sun S. 26° 20′ and S. 26° 24′.

Uranus is near β Virginis.

April 1, R.A. $11^h\ 44^{m}\cdot4$, Dec. 2° 33′ N., tr. $11^h\ 1^m$, sets $17^h\ 18^m$
30, 11 40 ·8, 2 56 N., 9 4 15 22

Total eclipse of the Moon on April 9–10, invisible at Greenwich. First contact with penumbra April 9, $20^h\ 42^{m}\cdot6$, with shadow $21^h\ 52^{m}\cdot5$; beginning of total phase April 9, $23^h\ 0^{m}\cdot3$, middle of eclipse $23^h\ 46^{m}\cdot6$, end of total phase April 10, $0^h\ 32^{m}\cdot9$; last contact with shadow $1^h\ 40^{m}\cdot7$, with penumbra $2^h\ 50^{m}\cdot6$.

Partial eclipse of the Sun on April 25, invisible at Greenwich. Begins on the Earth generally April 25, $1^h\ 0^{m}\cdot2$ G.M.T. in Long. 82° 10′ W. and Lat. 59° 9′ S. Greatest eclipse April 25, $2^h\ 46^{m}\cdot3$, in Long. 4° 38′ E. and Lat. 70° 52′ S., about three-quarters of the Sun's diameter being obscured. Ends on the Earth generally April 25, $4^h\ 32^{m}\cdot5$, in Long. 12° 25′ E. and Lat. 33° 6′ S.

Phenomena.

	G. M. T. h	m			G. M. T. h	m	
Apr. 1	9	1	J. i. Oc. D.	Apr. 14	7	53	J. iii. Ec. R.
	12	34	J. i. c. R.		10	17	J. ii. Oc. D.
2	8	39	J. i. Tr. E.	15	12	50	J. i. Oc. D.
3	8	50	J. iii. Tr. E.	16	10	8	J. i. Tr. I.
5	12	45	J. ii. Tr. I.		12	28	J. i. Tr. E.
6	9	45	J. iv. Oc. D.	17	10	54	J. i. Ec. R.
7	7	43	J. ii. Oc. D.	21	8	23	J. iii. Ec. D.
	13	6	J. ii. Ec. R.		11	53	J. iii. Ec. R.
8	10	55	J. i. Oc. D.	23	8	15	J. iv. Oc. R.
9	8	13	J. i. Tr. I.		10	13	J. ii. Tr. E.
	10	33	J. i. Tr. E.		12	3	J. i. Tr. I.
10	8	58	J. i. Ec. R.	24	9	15	J. i. Oc. D.
	9	12	J. iii. Tr. I.	25	8	52	J. i. Tr. E.
	12	46	J. iii. Tr. E.	28	10	49	J. iii. Oc. R.
13	13	13	B.A.C. 5408 Oc. R. 252°.		12	23	J. iii. Ec. D.
				30	10	0	J. ii. Tr. I.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

amongst them in no particular order, but breaking out irregularly in their vicinity. These sub-permanent spots sometimes lasted from two to three days, but at other times they disappeared in one or two hours; and they were never seen at a distance from the zones of ordinary sun-spots. In that respect they differed entirely from the veiled spots, which might be seen as far as the poles; and the assistant whom he generally employed to execute these drawings said that as a rule he had not the least difficulty in detecting whether the object was a veiled spot or a sub-permanent spot. In the latter case it had a more or less reddish appearance, and in the former case its colour was a cold grey.

Having watched the Sun as they had done from day to day, he thought it was possible to take perfectly accurate drawings; and he had brought a number of their original drawings in a portfolio for inspection. They used the ordinary method of projection, an image of the Sun ten and a half inches in diameter being thrown on a light board firmly attached to the telescope. The first duty of the assistant was to trace the outline of each spot and of its penumbra; he next placed his rapid sketch close by the sun-spot and then filled in every little detail he could. The imagination had nothing to do with it, and no mark was made on the picture which was not actually seen on the Sun's surface. As each detail was put in, the image of the Sun was moved by means of a slow-motion rod, and it was placed over the drawings so as to make one coincide with the other. If they got the best photographs which had been produced, and compared them with the details he could give in his sun-spots, he thought they would find that the drawings were as accurate as the finest pencil could make them. He certainly thought his assistant had succeeded very well indeed; but the great test would be to bring the picture side by side with negatives of the same size; and now he was glad to say they had negatives of 15 inches, 12 inches, and 8 inches, taken by Fellows of the Society who were present that evening, and he was afraid that they might condemn a good deal of his work; but he would be willing to hear their condemnation in order that he might improve upon what he had been able to attain to so far. He wished to compare these drawings with the best photographs, and then to see how far the outlines were incorrect, and how far the detail was fuller than that given by the photographs, for he was certain there was a great deal of detail in the drawings that would not be shown in photographs for several years to come. (Hear, hear!) What he wished principally to draw their attention to, were the faculæ; and he should be very glad indeed if any suggestion could be made for improving their representation. He had tried other colours as well as red, but found nothing succeeded so well as the red pencil; not that they imagined for a moment that the faculæ were red, for, of course, they were a brilliant white; but the red struck the eye, and gave, he thought, a better idea of what was seen in a telescope. He would be very grateful indeed if any one present could give him any information that would enable him to make better drawings for the future. (Applause.)

Mr. Knobel. Have you seen any shadows cast by the faculæ, as announced by Trouvelot?

The Rev. Father Perry. I have not seen exactly what Trouvelot has described. On two or three occasions I thought I saw something of the kind, but a more careful examination showed that it was not the case.

Major-General Tennant. Some time ago I devoted a great deal of time to the observation of sun-spots in detail, and especially to watching individual spots, and I can confirm Father Perry's statement that the changes on the Sun are extremely rapid. The impression produced upon me was that it was impossible to represent by any process of drawing the changes that took place, and that we could only secure a record of such changes by taking photographs of a small portion of the Sun in very rapid succession, and I shall be glad to see that carried out, because I am sure we have much to learn about the changes that take place in solar spots.

Dr. Gill. There must be some difficulty in obtaining drawings which will, well and accurately, represent the solar markings, but I think the process Father Perry has described is extremely good. To get the best results, however, I think we must have photographic representations on a large scale. The plan suggested by Dr. Vogel, and regularly carried out at Potsdam, is an extremely good one. A good telescope with a high power is mounted on the same stand as the photoheliograph, and the observer watches the Sun, and by means of an electric contact-piece exposes the plate in the photoheliograph at a moment of specially good definition. By-and-by, with the aid of some such process, I think the photographic registers of the appearance of sun-spots may be improved. It is desirable that these curious details should be photographed and carefully studied.

Mr. Brett. From a casual examination of these drawings I find they are extremely beautiful in execution; indeed it is scarcely possible to have better drawings. I would also remark that it is easy to exaggerate the accuracy of photographs. Photographs are liable to errors of their own, and to greater errors than drawings when these are taken by competent draughtsmen.

The Astronomer Royal. I think we may congratulate Father Perry upon the success of his observations. I have looked casually at the drawings, and have been struck by their great beauty. Although I am not disposed to undervalue photographs, still it seems to me there is ample scope for the draughtsman in delineating details of the solar surface and the changes in sun-spots. There appear to be two different classes of work to be done, one suitable for photography, namely the enumeration of sun-spots and the measurement of their areas and positions, and the other, the furnishing records of the changes that are taking place. In the present state of the subject these changes would be best discussed by means of drawings and eye observations. One thing must be borne in mind in regard to photographs. When a large number of photographs have been obtained, you have to discuss them and measure them; and the question arises whether for certain purposes it is not better to observe the Sun directly than to observe photographs, which necessarily give a more or less imperfect representation of the Sun. Therefore, there is very valuable work still to be done in this line, and we may congratulate Father Perry on having taken it up in this very successful way. I think it is of no use for amateurs to make pictures of the Sun showing the sun-spots on a small scale, with the view of giving their positions and areas, because that can be done better by photography; but, on the other hand, the work of recording minute changes in the detail of the Sun's surface, and in the structure of the spots can better be done by drawings made by eye observations of the spots than by photography.

Lieut.-Col. Tupman. What are the sizes of the veiled spots? I have never seen them, though I have stared at the Sun a good deal.

The Rev. Father Perry. I think I have already stated that no observer has seen them before, excepting Trouvelot, as mentioned by Professor Young. They are easily seen by those who know what they are looking for, but an observer watching for something else would overlook them. General Tennant has spoken about the rapid changes which take place in the spots. Any one watching the Sun for a long time will notice such rapid changes in the spots and in the faculæ as well. These apparent changes are not produced by passing clouds, because frequently there will be an exquisite definition of the spots side by side with a bad definition of the faculæ. There was one other point which I forgot to mention, and to which I should like to allude, and that is, that I have observed moving bodies apparently on the Sun. On twelve different occasions we have seen rapidly moving bodies appear on the disk of the Sun—not coming on at the limb—but moving generally from the vicinity of the great spots. These, as a rule, are not well defined; they move sometimes over a distance of 7 or 8 minutes of arc, and then disappear whilst still upon the Sun's disk. The path is sometimes curved. They disappear instantaneously, not by gradually fading away or by getting less well defined, because if that were so, I should say they had no connection with the Sun. Some of them are so rapid in their motion that they do not last more than a few minutes, and generally only a few seconds; and on account of this rapid motion I cannot believe they are actually on the Sun, though it may possibly be that they have some connection with it. But having observed these movements, I think they require some explanation, and I have brought them before this Society because its motto is, "Every thing you see must be noted."

Lieut.-Col. Tupman. How large are they?

The Rev. Father Perry. These moving spots subtend, I should say, about 7 or 8 seconds of arc. They are never very large. But as to the veiled spots, we have had instances where they have been joined together and have formed one continuous veiled spot, extending at least one-tenth of the solar diameter.

Lieut.-Col. Tupman. Have you seen Janssen's photograph?

Father Perry. I have examined some positives.

Lieut.-Col. Tupman. Are they like the rice-grains?

Mr. Ranyard. Do you mean the blurring or the rice-grains?

The Rev. Father Perry replied that they were apparently incipient spots, and sometimes appeared like darker interstices between the solar flames. A little cloud would come and remain for two or three minutes and then entirely disappear, whilst all the other features would remain exactly as before. They were entirely different from the blurring on Janssen's photographs.

The Astronomer Royal. These are not the veiled spots of Trouvelot?

The Rev. Father Perry said that the spots were of two kinds, one lasted 2 or 3 minutes, and the other 2 or 3 days. No observer before Trouvelot had recorded the first kind, and whether he had distinguished the other kind from the penumbra surrounding the dark part of the spot, he did not know. He (Father Perry) found these markings around the penumbra of the spots, and they were always ill-defined as if they were spots but half uncovered.

Mr. Ranyard. Are they as dark as the penumbra?

The Rev. Father Perry. Nearly as dark.

The Astronomer Royal. These veiled spots, as described by Trouvelot, were frequently photographed at Greenwich about the time of the last minimum of sun-spots, but I did not observe that they varied in this rapid manner. They had the character of ordinary spots covered over by the rice-grains or granulation of the photosphere.

Mr. Knobel. Are these veiled spots always seen near the ordinary spots, or anywhere disposed about them?

The Rev. Father Perry. The first class, which lasted a short time, are seen right up to the poles; there is no part of the solar surface where we do not detect them. The sub-permanent spots are seen always in the vicinity of the ordinary spots.

The President. Have these moving bodies on the Sun the appearance of rice-grains? You say you saw some of them in motion?

The Rev. Father Perry. The moving bodies had not the appearance of ordinary spots, nor of any thing else we ordinarily see on the solar surface. Sometimes they are nearly as dark as the umbra of an ordinary spot, but they are small. They are sometimes fairly well defined when they are first seen, and are fairly, but not quite sharply, defined when they disappear.

The President. Are you sure they are not something floating in our atmosphere, something like what Dawes saw some time ago? You know Dawes saw a number of floating particles.

The Rev. Father Perry. The bodies I saw did not pass off the Sun, but appeared and disappeared whilst still on the disk of the Sun, and that is the circumstance which requires explanation.

Mr. Maunder. Did Trouvelot notice that his veiled spots were evanescent?

The Rev. Father Perry. Professor Young says that Trouvelot saw them, but nobody else had seen them. He says it is very important the subject should be studied, but he does not give any details.

Lieut.-Col. Tupman. What aperture did you use?

The Rev. Father Perry. 8 inches, and a power of 20 at the outside. The diameter of the projected image was 10½ inches, but sometimes in the case of an interesting spot I magnified it still further.

Mr. Ranyard. You saw the veiled spots by projection?

The Rev. Father Perry. Yes, and my assistant, who is a skilled, accurate observer, sees them nearly every day.

Mr. Brett. Do you use a plaster-of-Paris surface to receive the projected image upon?

The Rev. Father Perry. No; the paper on which we make our drawings is placed on a small board.

Mr. Ranyard thought that Father Perry was referring to the blurred patches with which many were familiar, and which were well shown in Janssen's photographs; they were certainly very evanescent, though he did not know that there was any proof in the photographs that they vanished in a few minutes. He could not concur with General Tennant as to the very rapid change of the sun-spots, for they retained their general contour sometimes for months, and even those which changed more rapidly took some hours to make any large alteration. But those alterations were nothing compared to the alterations in the prominences; which was rather curious, when they took into account that the spots seem to be at a lower level, where the heat would be greater, and the changes should take place faster than at the higher level or top of the prominences, where the temperature would be less.

Major-General Tennant thought he had been misunderstood. He was fortunate enough on some occasions to see some very fine markings which resembled fine threads of cotton wool, but on returning again to it he had never been able to satisfy himself that he could see the same thing again. It was a small change of detail he referred to, not to changes in the general outline of the spot. He always saw in the details something he had not seen before. This had occurred day after day, and the changes were so rapid that there was not time to sketch them. That was the reason he would like to see photographic plates tried which would include every thing that could be seen, and then subsequent plates would show where any changes had taken place.

The President. We are much obliged to Father Perry for bringing forward a subject which has produced such an interesting discussion.

Mr. Knobel. I desire to call attention to a present the Society has received from Dr. Huggins. His photographic plates of the corona were placed in the hands of Mr. Wesley, who has made drawings of them, and these original drawings Dr. Huggins has presented to the Society. A sketch of the corona is also given from the photographs taken at Caroline Island. The rift shown in Dr. Huggins's coronal photographs taken in April and May 1882 are also shown in the photographs taken at Caroline Island during the Eclipse.

Prof. Pritchard read the following paper, entitled "On the proper Motions of 40 Stars in the Pleiades, both absolute and relative." He said :—The memoir, which I am about to present to the consideration of the Society, consists mainly of two separate and entirely independent investigations of the proper motions of forty stars in the Pleiades. It originated in a desire to test the capacity of a new differential micrometer which I had devised, under the expectation that it would enable me to measure, with great accuracy, celestial arcs to the extent of some twenty minutes, a distance which seemed to me considerably greater than could be reached with security by means of any filar micrometer with which I was acquainted. I purposed to apply this new form of micrometer, if successful, to a systematic investigation of stellar parallax, which I then intended to make an especial object of research in the observatory under my direction at Oxford. I applied the instrument, in the first instance, to the Pleiades, in consequence of a remark made by Bessel, to the effect that his own measures of the relative coordinates of certain members of this historic group had been effected with so much accuracy by means of his heliometer, that advantage might henceforth be taken of his scrupulous labours therein, to test the accuracy of other instruments, and, at the same time, assist in determining their scales. I had proceeded to a considerable extent with this investigation when, becoming satisfied with the capacity of the new micrometer, I resolved to still further utilize the observations, by widening the scope of the work, and by embarking on a systematic measurement of the relative positions of the more prominent members of the group, with the view of ascertaining their relative proper motions since the time of Bessel's heliometer measures, made some forty years ago.

The instrument in question affords the means of measuring arcs with equal facility in the three coordinate directions of right ascension, declination, and distance. Consequently, if all three were measured, there would arise at once the means of testing the accuracy of the observations. This seemed to me to be a great gain, and I have uniformly applied the test to the observations recorded in the present memoir. Such, then, are the circumstances under which the research commenced. As I proceeded with it and endeavoured to make it complete, the questions before me widened out, and new objects of research presented themselves in close connection with the original inquiry. It became desirable, for instance, to record the relative magnitudes or degrees of lustre presented by the members of the group subjected to the micrometer. Estimations made by the eye on these objects, viewed in the field of the telescope, seemed to me to be eminently unsatisfactory. In support of this remark I may mention that out of estimations made by eminent observers, such as Flamsteed, Bradley, Mayer, Piazzi, Bessel, Argelander, and Wolf, no two agree in the order of the relative brightness of the eight principal stars in the group. These considerations led me to the invention of the photometric method of measurement of relative brightness, by means of the "*Wedge Photometer*," which has already issued in the memoir on the subject printed in the last volume of the Society's Transactions, and which I trust before long to complete as a new *Uranometria* of all the stars visible to the naked eye from the pole to the equator. In the course of the reduction of the micrometer measures another question also not unnaturally arose. The results thus obtained are from differential observations; but scattered about in the records of many observatories, from the time of Bradley downwards to the present time, there are numerous observations of several members of the group made with meridian instruments, and affording reliable measures of their absolute right ascensions and declinations. If, therefore, these valuable observations, made at different epochs and by various observers of great eminence, were reduced on a uniform and systematic plan, it would thereby be possible to deduce the present coordinates and the proper motions of the objects to which these observations refer. The work, I felt, would be somewhat laborious; but it would supply the means of an interesting and somewhat novel intercomparison between the results obtained by a long series of meridian observations, with those accruing from differential measures made away from the meridian. Accordingly I was led to undertake the task, and the numerical results are complete and ready for presentation to the Society. There are fourteen stars in the Pleiades whose places have been obtained by the meridian observations referred to. They extend for the most part through a period of 130 years, and are worthy of various degrees of reliance. In systematically effecting their reduction I have mainly followed the plan pursued by Argelander in the 7th volume of the Bonn observations, largely availing myself of the facilities afforded by the later labours of Professors Newcomb and Boss. The result of this section of the present research consists in the determination of the most probable absolute coordinates and proper motions of the fourteen stars in question. These proper motions thus obtained from the best existing meridian observations are then compared with those resulting from micrometrical measurements. For the latter purpose recourse is had to the heliometer measures made at Königsberg between 1838–41; to a very valuable set of differential measures made at Paris by Dr. Wolf in 1874 &c.; and to the measures made at Oxford with the duplex micrometer between 1878–80. In addition to these there is another apparently valuable set of measures made at Washington about the year 1860. These observations, however, do not appear to have been systematically reduced. In the Washington Catalogue for 1860 the absolute co-ordinates are given; but the information there supplied is, I regret, not sufficient to enable me to combine them with the other series of relative coordinates here mentioned. These three sets of observations are combined together by means of equations of condition; and, as in the case of the afore-mentioned meridian work, furnish at once the most probable relative coordinates of the forty stars in question, together with their relative proper motions. The final results of these investigations are as follows:—First, with regard to the fourteen stars which have been measured by the two thoroughly independent processes, the accord of the final results points unmistakably to the general existence of certain small motions of these stars *inter se*; but to so slight an amount, that their exact determination does not seem within the reach of observations as yet extant, but must be left for the effects of a greater lapse of time, which shall make them more directly apparent in amount, and to the astronomers of the future. Of the remaining stars, although meridian observations do not at present exist so as to corroborate the evidences of relative motion afforded by the combined micrometer measures made at Königsberg, Paris, and Oxford, nevertheless the fact of these minute displacements is, I think, now placed beyond doubt. An independent confirmation of these results is also obtained from a comparison of *distances* measured at Königsberg and Oxford. Another remark also occurs, which is this—the relative displacements of these distant suns, although not distinctly and absolutely measurable, appear to vary both in direction and amount, indicating thereby the mutual influence of a group of gravitating bodies, and not simply that apparent common motion of the whole which would arise from the translation of the solar system in space*.

Dr. Gill. I think this paper of Professor Pritchard's should not be passed over without some remark, because it really is the foundation, as it seems to me, of a new series of investigations that bear in a most interesting way upon Astronomy generally. It is due to England to say that Sir William Herschel discovered the orbital motions of double stars; and if Professor Pritchard's results are what they are described to be, we have here the first positive proof of the mutual connection of the separate objects of

* In further connection with the completion, for the present, of this very interesting inquiry into the observable motions of a cosmical system, I may take this opportunity of stating that I have laid the foundation for a similar inquiry by making with scrupulous care, in the University Observatory, a series of observations of the relative positions of some two hundred and fifty stars of a cluster in Cygnus (39 Messier). I hope in due time to communicate the results to the Society. Perhaps these measures, after the lapse of a century, may, like Bradley's observations, made a hundred and thirty years ago, enable some astronomer of the future to trace the effects of gravitation in a system of suns possibly detached from our own.—C. P.

a star-cluster. The precise degree of accuracy that has been arrived at can only be decided by careful study of the figures; but in the hands of Professor Pritchard the bare statement is worth a very great deal, and we may congratulate him upon a result which is extremely interesting and important.

A Fellow. No study is more interesting in Astronomy than that of star-clusters. There is not only the cluster Prof. Pritchard has been speaking of, but the one in Perseus, and many others of the same kind.

Lieut.-Col. Tupman exhibited a drawing of Saturn made by Mr. Pratt.

Mr. Knobel. During the last winter the planet Mars has come to opposition, and this opposition has been different to those that have preceded it during the last few years, on account of the northern hemisphere having been turned towards the Earth, at which time the planet is of small diameter. I have made a series of sketches of the planet this winter, which I have presented to the Society, and which presents certain features of novelty suggesting alterations in our existing charts. This winter there have been some nights of remarkably fine definition, and more detail has been seen than on previous occasions when the northern hemisphere has been turned towards the Earth. On the night of Feb. 29 I was able distinctly to see two lines very similar to the canals which have been delineated by Professor Schiaparelli—one of them the canal he calls Cyclopum, and another I think I can identify as one in his chart of 1879. The most remarkable point is, that I was able to see that a large portion of the surface was covered apparently with reticulated markings. The reticulation was very distinct; but it was not like the straight parallel canals in Schiaparelli's observations in 1881. There is a dark shade in some of the drawings of this part of the surface of the planet, but on this occasion that was not visible; and one is led to think there may have been clouds partially concealing the shaded surface, and that that gave rise to the remarkable phenomenon I observed. There were some other features, but I will not occupy time by mentioning them now.

Mr. Brett. It is rather invidious for me to be called upon to criticize other people's drawings. We all know the difficulty of making astronomical drawings. Mr. Pratt's drawing of Saturn is extremely interesting and important; and if it were presented to the Society we should all welcome it with very great pleasure. It has, of course, defects as all drawings have. It is slightly out of perspective, and is not lighted quite correctly as a sphere; but leaving these technical objections aside, it is a drawing of very considerable importance and merit.

Mr. Common read a "Note on a method of reducing the Friction of the Polar Axis of a large Telescope." This he proposed to effect by floating it in a bath of mercury, or, in the case of a very large telescope, of water.

The President. Dr. Gill and Mr. Lockyer have both made some recent visits to observatories on the Continent; and we should be glad if they would give us some account of what they have seen.

Dr. Gill. I have just returned from a short and hurried tour on the continent. I have had the pleasure of visiting the workshops of Messrs. Repsold, and the observatories of Berlin, Potsdam, Bonn, Strassburg, and Paris. One could say a great deal about each; but I will say but little just now, as I hope another time to go more into detail. I saw at Hamburg the mounting of the great 30-inch equatoreal which is being constructed by Repsold for the Pulkova observatory. It is one of the first equatoreal instruments that I have seen on the continent that presents what we in England should regard as a reasonable amount of stability. It is a very powerfully mounted instrument, and has strong hollow steel polar and declination axes; and the tube, contrary to our usual form, is a *parallel* tube. It is not a cone; and the reason why Repsold has not made it a cone is that they do not wish to set the axis of the tube too far from the polar axis. It is a *parallel* tube, and the centre part is made of cast iron; and in order to minimize the distance from the polar axis the actual attachment of the polar axis of the tube is sunk into the central axis, so as to give the greatest possible rigidity. The steel plates diminish in thickness in each successive layer towards the object-glass, and the result is that the whole structure is extremely rigid. The remarkable feature of the mounting is that the instrument can be set both in declination and R.A. by the assistant who is seated at the bottom of the polar axis, and by looking into the bottom of the polar axis he can read the declination and hour-circle. The instrument is 55 feet focal length. The micrometer is an instrument of cast iron, in all about a foot long, in order to get a sufficient field of view. It is of the usual form of Repsold's construction and has extraordinary perfection in details. They have also introduced a prism which can be thrust forward into the axis by an easy motion, so that though the primary object of the instrument is micrometer measures, yet by thrusting this prism forward into the axis, you get by two reflections the rays diverted at pleasure into a spectroscope mounted parallel to the tube, so that if a comet appears suddenly its place may be first determined, and then its spectrum examined, or *vice versâ*. This instrument is rather a compromise between the wishes of Otto Struvé and his son. Otto Struvé wished to have an instrument principally for micrometer measures, and his son for spectroscopic observations. The instrument is one that deserves very careful consideration and study. At Berlin I saw a new declinometer, an instrument by which you can register the difference of declination. A single wire is moved rapidly in declination by a screw with a coarse thread. By pressing the foot upon a little India-rubber ball placed between convenient pressure-boards, the observations in declination are recorded. The ball is connected by an India-rubber tube with an expansible metallic box, like that of an aneroid barometer, mounted in the micrometer. By pressure of the foot on the little India-rubber ball, a slip of paper is brought by the lid of the box into contact with type-wheels, which are turned by the micrometer-screw. One of these wheels indicates the number of whole revolutions, the other the one-hundredth parts of revolutions, and there is between the two wheels a fixed line of reference. You thus, by a pressure of the foot, print upon a slip of paper the readings of the screw. The same action causes the roll of paper to move on automatically, ready for the next impression. The Right Ascension is recorded on an ordinary chronograph, the magnitude being recorded by a number of dots after the observation. One observer can thus do zone work very completely and rapidly. I saw a very ingenious form of self-recording barograph, the principle of which was the weighing of a column of mercury, and I also saw a thermograph on a similar principle. I met with great kindness from all the astronomers at Berlin. I visited the observatory for national measures, and examined the means of comparing the standards of weight. At Potsdam I was much interested in seeing the very beautiful observatory chiefly dedicated to physical research, by the liberality of the German government, at the head of which my old friend Dr. Vogel holds his very able sway. I cannot say I am satisfied with the stability of the equatoreal mounting. The Great Equatoreal is not what Englishmen would call a stable instrument. It is very beautiful in its mechanical details, but wants that rigidity and stability which we all like to see in our equatoreals. Some of the spectroscopic contrivances which Dr. Löhse has in operation are very beautiful, particularly that with which he measures directly the wave-length of any particular line with a single prism adjusted for minimum deviation. There is also a contrivance which he has, in which, by means of two gratings, he measures the displacement of lines in the spectra of stars so as to determine their motion in the line of sight. At Bonn I saw one of the finest illustrations of the power of man over an instrument. One of the largest astronomical works that we have going on in Europe has been brought almost to completion by Professor Schoenfeld, with an equatoreal of six inches aperture, but with a mounting so vile that it would make an English amateur shiver to look at it, illustrating the saying of Professor Watson, that the most important part of an instrument is the man at the small end. He has just brought to completion his scheme for carrying the 'Durchmusterung' of Argelander from 2° south of the equator to 25° south, and I have seen one of the charts. I got much practical information from him as to the methods by which such a work could be carried out, and I hope at some time to put some of the good advice into practice at the Cape. At Paris I was particularly interested with the Loewy Equatoreal Coudé. I was very anxious to test the definition of the instrument. It depends on two reflections from two plane mirrors. There can be no question whatever as to the practical convenience of sitting in a comfortable room on a cold night, instead of being exposed in an observatory. The instrument is absolutely rigid, and the wind may blow and roar, but you can observe comfortably. One fine night, about 11 o'clock, we went to the observatory, and set on γ Leonis; and I am bound to say I never saw the diffraction disks of a star better defined than in that instrument. They were perfectly circular. The disks came as sharply to focus as any I ever saw, and I would not have believed, if I had not seen it, that it was possible to make an instrument in which, after two reflections, such definition could be found. I am bound to say I never saw better definition in any instrument, and I never measured a double star so pleasantly and easily before. Whether the amount of light lost is very large, I cannot say; but Mr. Loewy tells me that from photometric observations he finds that the loss of light is equivalent only to that of half an inch in diameter on the 10½-inch object-glass; and if that is so, I think the Equatoreal Coudé is very valuable. I saw also in course of construction in Paris a new instrument for the observatory of Nice; and probably Mr. Lockyer, who has been at Nice, will say something on that subject. I also saw the Transit instruments which are being made for French observatories. They present some peculiarities, and I should be glad if we could have a discussion on these instruments in connection with Dr. Copeland's paper. Another thing that struck me very much was the enormous progress made in the establishment of observatories in France. There are eight public observatories being established, but there are at present more instruments and observatories than observers; but the French say, with great truth, that before you can get men to study astronomy, you must have places to put them in, and you cannot expect men to devote their life to astronomy without the prospect of being employed, and therefore they create the observatories before they create the men. There are four young men ready to emerge from the School of Astronomy in Paris.

Mr. Lockyer. There are one or two points only, after what Dr. Gill has said, on which I should like to say a few words. I was glad to hear Dr. Gill's opinion of the Equatoreal Coudé, because I must say I think it is one of the instruments of the future. I say this because, at the works of Messrs. Henry, I saw a plane mirror of 40 inches diameter, which some British opticians have told us it is impossible to make. It is finished, and is stated to be as perfect as the mirror at present used by Loewy for his instrument. If it were a question of mounting a 27- or 28-inch telescope, I would make it like the Equatoreal Coudé, for a reason which Dr. Gill did not go into, viz. that of the brutal finance. There would be several made in France if the crown glass could be got, but it is not to be got; and I am afraid the manufacture of optical glass in France is on the eve of destruction. But if these instruments of 27 or 28 inches aperture were made on the ordinary plan, the observatory for each of them would cost £20,000. Two observatories which are being built for these instruments, which are not yet made, in consequence of the lack of crown, are estimated to cost half a million francs each. The observatory at Nice will cost very much more, because Mr. Bischoffsheim wishes it to be a little monumental. So that when you have your Equatoreal Coudé of 27 inches aperture, cost what it will, you will not want a dome which, according to the French standard, will cost £20,000. It occasionally happens that even in England there is a difficulty in getting money for scientific purposes; and if you could get a mounting which will reduce the cost of the dome to a few hundreds, then you have something more than the diffraction disks to consider. At Nice, where I had the pleasure of stopping for a week, we were requested by Mr. Bischoffsheim to form ourselves into a committee to consider what is best to be done next. The view which was much in favour there was to get a telescope as large as possible for physical work, and convenience was to be secured by means of plane mirrors arranged in a way I need not explain, but in a way which would have reduced the light considerably. I ventured to suggest (and the more I think of it, the more I like the suggestion) that, considering what Dr. Huggins and Mr. Common have done, we should now in such work abolish the observer altogether. It may at first seem revolutionary to say so; but I think any astronomer would be losing his time now if he were to attempt to draw the nebula of Orion, or to draw the spectra of stars which are recorded on the photographs which Dr. Huggins has given us. These things can now be better and more permanently done by photography. If we are to do any thing with reference to the people who come after us, we want a catalogue of the existing state of the heavens in the most convenient form; and I picture to myself an astronomer 1000 years hence in a room filled with photographs giving a picture of every part of the heavens from the north pole to the south, taken in the 19th century. This could easily be done by abolishing eye-observations; and with this enormous advantage, that you could also abolish the dome, and hence two thirds of the expense. Now I ascertained that in France a telescope of 8 feet aperture and 40 feet focal length can be produced in 10 months for £10,000. Such a telescope could be protected, when not being used, in an observatory which need not cost £100. The telescope can be charged with 12 photographic plates, which can be exposed by means of an electro-magnet, and you can in this way get more rapid pictures, with less labour, than by the ordinary means of observation. I hope the Fellows will agree with me that it is our duty to do something to provide such a celestial library for those who come after us. Sir John Herschel many years ago gave out the same idea; but I think our responsibilities are very much greater now than his were, because since his time better and more convenient methods have been elaborated for obtaining such a library as he suggested.

A vote of thanks was awarded to Dr. Gill and Mr. Lockyer for their explanations.

The following papers were also announced:—

J. Tebbutt. "Elements of the Orbit of Comet *a*, 1884."

Dr. J. Morrison. "The Orbit of Pons Comet."

Prof. A. Hall. "Observations made at the U.S. Naval Observatory, Washington."

Prof. A. Hall. "The Motion of Hyperion."

Capt. G. F. Parson. "Sextant Observations of Comet Pons-Brooks, made on board the ship 'Earnock.'"

Prof. G. W. Hough and *S. W. Burnham.* "Observations of the Companion of Sirius made at the Dearborn Observatory, Chicago."

Dr. C. L. Prince. "On the Occultation of κ Cancri."

E. Neison. "On the Corrections required by Hansen's 'Tables de la Lune.'"

J. Macquire. "Note on the Eclipse of Thales."

Capt. D. W. Barker. "Sextant Observations of Comet Pons-Brooks, made on board the ship 'Superb,' Jan. and Feb. 1884."

Capt. A. S. Thomson. "Sextant Observations of Comet Pons-Brooks, January 1884."

Dr. R. Copeland. "Suggestions for the improvement of the Transit Circle."

E. J. Stone. "Observations of the Moon made at the Radcliffe Observatory during the year 1883, and a Comparison of the Results with the Tabular Places from Hansen's Lunar Tables."

The following gentlemen were elected Fellows of the Society:—Charles Michie Smith, B.Sc., and Arthur Stanley Williams.

The Meeting adjourned at 10^h 5^m P.M.

The Variable Star R Leonis.

The late Professor Argelander in his discussion of the observations of this variable, in Vol. vii. of the 'Bonner Beobachtungen,' obtained for the epoch 1855, Oct. 9·24, a mean period of $312^{d}\cdot1894$, but found that during the interval over which the observations extended a sensible shortening of the period had taken place. My own observations during the 24 years 1840 to 1864 give results agreeing very fairly with those derived by Prof. Argelander from the observations which he discussed; but those made since show that during the last twenty years, 1864 to 1884, a decided lengthening of the period has taken place. The dates of maxima derived from my observations are as follows:—

1840, May 23	1859, Mar. 12
1848, Dec. 12	1860, Jan. 14
1849, Oct. 15	1864, Apr. 21
1852, May 7	1865, Mar. 5
1853, Mar. 16	1866, Jan. 27
1854, Jan. 27	1882, May 25
1854, Dec. 6	1883, Apr. 5
1855, Oct. 13	1884, Feb. 13
1858, May 6	

Forming the usual equations and treating by the method of least squares, the mean period from May 23, 1840, to April 21, 1864, = $312^{d}\cdot125$; and from April 21, 1864, to Feb. 13, 1884, = $314^{d}\cdot375$. It appears therefore that the mean length of the period has been greater during the last 20 years than in the previous 24 years; but, as Argelander had remarked in the case of the shortening of the period, the rate of lengthening has not been proportional to the time; and it is to be remarked that the increase of length has been greater than the previous decrease, as shown by the observations discussed by Argelander. It is important, therefore, that a careful watch should be kept upon this finely coloured variable, and comparisons frequently made with neighbouring stars whose magnitudes have been carefully determined photometrically.

The Observatory, Birkdale, Southport, 1884, May 14. JOSEPH BAXENDELL.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Shadows cast by Faculæ.

SIR,—

After reading the note in your May Number, on "Shadows cast by Faculæ," while at the new observatory lately erected by the late Mr. Thomas Coats for the town of Paisley, I thought of examining a large spot now near the Sun's western limb to see if it showed any thing of the kind reported by M. Trouvelot. On examining it at 6^{h} G.M.T., May 2nd, with the Dawes eyepiece on the $5\frac{1}{2}$-in. Cooke equatoreal, I distinctly saw a shading of the kind figured in your engraving. The observer, Mr. Maclean, on looking, at once saw it also, and it continued very distinctly visible as long as we watched it. It was equally distinct with three or four powers, and various diaphragms and glass shades. I enclose a careful copy of a drawing I made at the time. A B is the shade

referred to, and at the point C it accurately followed the outline of the intensely white margin of the spot. This margin was equally brilliant all round the spot, and was connected with the network of faculæ figured in the neighbourhood. Round the rest of the spot margin, beyond A and B, there was no shade, the penumbra coming uniformly up to the brilliant edge.

Barrhead, Scotland,
1884, May 3rd.

Yours faithfully,
EDWARD B. KIRK.

Vulcan.

SIR,—

In the 'Observatory,' vol. i. p. 26, it is stated that, according to M. Le Verrier, we must wait at least six years from 1877 before a transit of Vulcan across the Sun could possibly occur.

The time has therefore now arrived when transits should be looked for. From Chambers's 'Descriptive Astronomy' we learn that they can only occur between March 25 and April 10 at the descending, and between September 27 and October 14 at the ascending node, on Le Verrier's hypothesis.

Accordingly frequent observation of the Sun's disk was made here this year in March and April, without, however, any planet-like object being seen on it.

Should not attention now be drawn to systematic international observations, with a view to ascertaining the real existence of the planet by its appearance in transit over the Sun?

Yours truly,
Sunderland, 1884, May 1. T. W. BACKHOUSE.

Period of U Cephei.

SIR,—

Owing to an oversight on my part, there is unfortunately an error in the period of the variable star U Cephei, deduced from my own observations, as given in the last number of the 'Observatory.' Instead of 2·492857, the numbers should stand 2·492872 days. The corresponding epoch is 1882, April 19·926414.

In my letter I referred to the magnitude difference which seems characteristic of alternate minima. An examination of the differences between the observed times of minimum and the times calculated with the elements given above introduces another point of interest (to which my attention was first called by Mr. Baxendell), viz. that there is a much greater time-irregularity in the high than in the low minima. The mean error of a *low* minimum is 2·39 minutes, while that of a *high* minimum is 8·55 minutes, between three and four times as large. A remarkable instance of this irregularity is found in the case of the minimum of November 27, 1880 (not observed by me, but well observed by Mr. Baxendell, who kindly allows me to quote it), when the minimum occurred nearly 41 minutes earlier than the calculated time.

It is thus evident that there are points of interest in this star, and especially so in respect of minima of this class, which are well deserving of careful investigation.

Knowles Lodge, Cuckfield,
1884, May 15.

I am, Sir, yours faithfully,
GEORGE KNOTT.

NOTES.

'BULLETIN ASTRONOMIQUE.'—The new French astronomical monthly, the appearance of the first number of which was noticed in the 'Observatory' for March (No. 83, p. 85), calls for a fuller notice than we were then able to accord to it. The 'Bulletin Astronomique' is published by M. Gauthier-Villars, and in paper, typography, and general appearance sustains the high reputation of that well-known house. The periodical is divided into two sections, the first consisting of articles by various astronomers (principally those attached to the Paris Observatory) on various astronomical subjects and of the details of recent observations. Perhaps these details might have been better omitted to provide room for more important matter; for the observations of the place of a comet or minor planet would be more useful in the 'Comptes Rendus' or 'Astronomische Nachrichten,' where they have hitherto been published, than in a monthly publication like the 'Bulletin.' But the second section is a novelty, and promises to be a most useful one. It is to be devoted to notices of current astronomical news, and a complete *résumé* of the chief astronomical periodical publications and of newly published memoirs. Thus every paper read before the Royal Astronomical Society at its last Meeting is named, and where necessary an abstract is added, and the report of the discussion carried on at the Meeting is abridged from that given in the 'Observatory.' The following astronomical publications are also summarized—'The Monthly Notices,' 'The Observatory,' 'The Astronomical Register,' 'The Sidereal Messenger,' 'Copernicus,' 'Astronomische Nachrichten,' and the astronomical papers in the 'American Journal of Science.' If a yet further section could be added, giving notes as to the progress of new and unpublished investigations, the 'Bulletin' would afford a most complete view of the state of the science at the actual moment. We trust that M. Tisserand and his *collaborateurs* will find themselves able to carry out their scheme with the ability and thoroughness with which they have commenced it, and that their efforts will meet with the appreciation and success they so well deserve.

COMET 1858 III.*—M. Schulhof has been lately reinvestigating the observations of this comet, discovered by Mr. H. Tuttle on 1858 May 2. The comet was not seen in Europe, and only eight

* Astron. Nachr. No. 2590 and 2592.

observations were obtained of it at Cambridge, U.S., and Ann Arbor. These observations, however, M. Schulhof finds are closely represented by an elliptical orbit with a period of about 6½ years; but the actual period remains somewhat uncertain, since M. Schulhof has been unable to find a satisfactory instance of its observation at any other return, though the elements of the comet of 1092 bear some resemblance to those of the comet in question; and it may possibly have been observed as a nebula by Sir John Herschel, since there is one, No. 2094 of his general catalogue, which he observed once and was not able to find again, the place of which might correspond to that of this comet at that time. Notwithstanding the great uncertainty in the period of revolution, M. Schulhof is publishing sweeping ephemerides for the interval from December to July. Should the comet come to perihelion in this part of the year, the conditions of visibility are comparatively favourable, and a systematic search undertaken each year according to these ephemerides should result in its rediscovery. It will always be necessary to carry the search over a wide zone, on account of the uncertainty as to the inclination of the orbit and the position of the node, and also on account of the perturbations occasioned by Jupiter, to which, according to the most probable elements, the comet must have approached very close in 1879 and 1880.

The following are M. Schulhof's elliptic elements for the comet:—

Perihelion Passage 1858, May 2·95896, Paris Mean Time.

Longitude of Perihelion	200° 46′ 27″·1	Mean Eq. and Ecl. 1858·0.
Longitude of Node	175 4 8 ·5	
Inclination	19 30 2 ·0	
Angle of Eccentricity......	41 21 5 ·2	
Mean Daily Sidereal Motion	536″·881	
Revolution	6·61 years.	

The late Prof. Moesta *.—This astronomer, who was formerly Director of the Observatory at Santiago in Chili, died at Dresden, after a severe illness, on the 2nd of April last, in the fifty-ninth year of his age. He was born at Zierenberg, near Cassel, Hesse, on the 21st of August, 1825, and studied mathematics and astronomy under Prof. Gerling at the University of Marburg. He went to Chili in 1850, where Gilliss, as chief of an astronomical expedition sent from Washington by Maury in 1849 (principally for the purpose of making a fresh determination of the solar parallax), had erected a provisional observatory and already commenced the observations. Soon after his arrival at Santiago, Moesta, after first working for a short time at the land-triangulation under Pissis, took part in these observations, which were

* Astron Nachr. No. 2588.

made at Cerro de Santa Lucia, a hill situated to the east of Santiago and about 200 feet higher. When Gilliss returned to Washington in 1852, the Government of Chili purchased his instruments and the wooden building he had erected for their reception; and on the 20th of August in that year, Moesta was appointed Director of the Chilian National Observatory, and also Professor of Astronomy and of the higher Mathematics at the University. He carried on his observations at the provisional observatory until 1860, and the results were published in two successive volumes. They include places of planets, of the southern stars of the British Association Catalogue, of Lacaille's stars, aad of small stars between the zenith and 62° south declination. The first volume (which embraces the work of three years) contains also observations of the total solar eclipse of the 30th of November, 1853. The Government of Chili decided in 1857 to erect a new observatory on the plan proposed by Moesta, which was completed in 1860. The greater part of the observations made there have been published in different volumes of the 'Astronomische Nachrichten; those of Mars at the opposition in 1862 appeared in a separate treatise. Whilst at Colina, a few miles north of Santiago, where he had gone to take the waters in 1865, he discovered, on the 18th of January, the first comet of that year, which was visible only in the southern hemisphere. Later in that year he received a commission from the Chilian government to procure in Europe a refractor of 9 inches aperture for the observatory. This he executed, and sent over the instrument, but did not return to Santiago on account of increasing ill-health. After spending some time alternately at Marburg and Cassel, he took up his abode at Dresden in 1870, reducing and publishing the results of his more recent observations at Santiago, partly in 'Annales de la Universidad de Chile,' and partly in volumes of the Astr. Nach., the last of these being the 99th. By special request he attended the meeting of astronomers held in Paris in 1881 to discuss the means of preparing for the transit of Venus in December of the following year, and gave information concerning the most suitable stations in Chili and Peru. W. T. L.

Hongkong Observatory.—Sites near the city of Victoria being unsuitable, Mount Elgin, a small hill of decomposed granite, which rises abruptly from the surrounding level ground of Kaulung peninsula and culminates in two humps about 100 yards apart, was selected, and on one of these, which has a circular flat surface about 200 feet in diameter, a rectangular building, 83 ft. by 45 ft., has been erected, the upper floor of which forms the residence of the astronomer and the lower the Observatory proper. On the ground floor are four rooms, each 20 ft. by 16 ft. and 14 ft. in height; one forms the official room of the director, a second the general office and computing-room, the others being used for the clocks and self-recording meteorological instruments; in addition to these there are the transit-room 14 ft. square, with an aperture for observing 1 foot in width, and a smaller room used as a photographic laboratory. A turret rises 8 feet above the upper storey for the self-recording anemometer, the cups of which are 45 feet above the ground. A sunshine-recorder is placed at 34 ft. above the ground, on the parapet of the main building; and the stands for the meteorological instruments are about 75 ft. to the S.W.

On the other eminence is a hut 17 ft. by 13 ft. and 11 ft. in height, in which are deposited the magnetic instruments, bamboo chips having been used in lieu of nails in the construction of the hut and furniture. A broad road connects the hut with the main building; a small house for the assistants and a dome for a 6-in. refractor (to be lent by the Astronomer Royal) are to be constructed beside the road.

The instruments at present in working order are a Transit by Messrs. Troughton and Simms, furnished with a delicate level for observing zenith-distances by Talcott's method. The Mean-Time clock, for dropping a time-ball 6 ft. in diameter, to be erected at Tsim-sh'at-sui Point, can be accurately adjusted to correct time by magnetic appliances. The Sidereal clock (complete with every modern improvement) is connected with a dial in the Transit-room. The clocks and apparatus connected therewith are by Messrs. E. Dent and Co. In addition to the usual thermometers &c. are a self-recording barograph and thermograph, the former placed on a stand screwed to the floor, and the latter supported by massive blocks of wood fixed on solid masonry. A dip-circle and unifilar magnetometer are placed on massive blocks of teak-wood sunk 3½ ft. in the ground and rising 4 ft. above it.

Tri-diurnal meteorological observations were started on Jan. 1 last, and daily weather reports based on observations made at the treaty-ports, Manilla, Nagasaki, and Wladivostock, at 10 A.M. and 4 P.M. on the previous day. The deviation is 47 minutes E. and the dip 32° (north end dipping). The buildings have been erected according to the wishes of Dr. Doberck throughout.

Dark Transits of the Satellites of Jupiter.—At the Meeting of the California Academy of Sciences held 1884 March 3, the President, Prof. Davidson, communicated some observations on recent transits of the third and fourth satellites of Jupiter. On Jan. 15, Prof. Davidson saw the third satellite and its shadow transit across the planet, passing along the dark brown red belt which has been persistent on Jupiter for some time past, yet both were markedly black. The satellite at one time, under a power of 255, was seen as a circle with two thirds of the disk bright and the other segment of one third the disk dark or black. On Feb. 24 Mr. Burckhalter observed the fourth satellite to enter on the disk, and noticed nothing unusual about it, the bright satellite being seen on the white belt with difficulty. But some hours later, on referring again to the telescope, the satellite, though on the same white belt, appeared as " a black spot, as black as a drop of ink." Mr. Hill, independently observing at another observatory, saw the dark satellite when it had nearly crossed the disk. It was seen with difficulty when close to the limb as a dusky spot, and its egress from the planet was watched.

Mr. Charles Todd, Government Astronomer at the Adelaide Observatory, writes concerning a later transit as follows:—

"On the evenings of March 20 and 21 Jupiter kept us quite busy. On the first evening there was a transit of the third satellite and an occultation of the first. The third satellite, which for a time after ingress appeared as a small bright disk, became quite black or dusky throughout its passage over the face of the planet, disappearing before reaching the western limb, although transiting the red belt, immediately to the west of the large hollow in the belt before mentioned as over or to the north of the great red spot, the outlines of which I could occasionally just faintly glimpse. I have seen the fourth satellite as a black spot on the planet, but never the third, and it is, I believe, a very rare occurrence. The black spot representing the satellite was not nearly so large nor so black as the same satellite's shadow, showing that it was probably due to defective reflection over the central portions of the satellite."

On May 19 Mr. Denning, we learn from 'Nature,' observed a dark transit of the first satellite, a somewhat unusual observation. The light of the satellite must have undergone a considerable diminution from March 21, on which occasion Mr. Todd states that, although it transited the equatoreal bright belt, he could keep it in view throughout the whole period of transit. On the previous evening, March 20, Mr. Todd had been able to see this, the first, satellite, at its occultation disappearance, " for about two thirds of its diameter through the edge or limb of the planet, but it wholly disappeared at the observed instant of internal contact."

Polar Spots on Venus*.—Some four years ago, M. Trouvelot published a short account in the 'Observatory,' Vol. iii. p. 416, of two remarkable white spots on Venus, on the opposite limbs of the planet and strongly resembling those seen round the poles of Mars. These were first observed on 1877 Nov. 16, since which date M. Trouvelot has reobserved one or other of them on 242 occasions, and has made 122 sketches of them. As the rotation of Venus on her axis does not seem to affect them to any sensible degree, and as, according to appearances, they remain in the same place during entire months, there is reason to suppose that the axis of the planet passes through or near to their centres. M. Trouvelot does not consider that these spots, which appear to him to be permanent or nearly so, are of quite the same character as those of Mars, for

* Comptes Rendus, Vol. xcviii. No. 12.

on one occasion, in 1878, the southern spots seemed to be composed of a multitude of bright peaks, forming on its northern border a row of brilliant star-like dots of light.

This appearance and the absence of any shading on the part of the spot crossed by the terminator would seem, M. Trouvelot considers, to indicate that the spots were the summits of high mountains rising above the cloudy envelope which covers Venus.

M. Trouvelot on the Planet Mars*.—M. Trouvelot, in a note communicated to the Academy of Sciences on 1884 March 31, states that the series of observations of the planet which he has been carrying on during the last nine years are now nearly completed, every part of the planet's surface having been carefully studied. There remains only the region which yet remains covered by the white north polar spot; this spot if it melts with the same rapidity as the white spot round the south pole, of which M. Trouvelot says, "j'ai plusieurs fois vu disparaître *complètement* la tache polaire australe" (a statement which many observers of Mars will consider to need some qualification), should disappear about the middle of August. M. Trouvelot has endeavoured to make his observations as perfectly continuous as possible, only intermitting them when the planet was lost to sight through its nearness to the Sun. He has accumulated no fewer than 415 careful drawings during these nine years' work and has a valuable collection of notes on the phenomena of the planet's surface or of its atmosphere. When therefore the disappearance of the north-polar snows shall have revealed the minute portion of the planet which yet remains unexplored, M. Trouvelot will have abundant material for the construction of a most complete map from his own observations.

M. Trouvelot remarks that "the northern hemisphere of Mars is much less rich in dark spots than the southern; besides the seas of Knobel, Tycho, and Airy of Mr. Green's chart, and the dark spots which now surround the polar cap, and of which I have recognized several branches extending towards the south, there are none of importance. The great continents of this hemisphere are, however, occupied by grey spots, more or less faint, which are scattered over them. Judging from the changes which I have seen these spots undergo from year to year, it might be thought that the variable grey spots are due to a Martial vegetation undergoing the alternations of the seasons." M. Trouvelot adds that he has observed very important changes in the spots of the southern hemisphere; and particularly instances the region to the north of Terby Sea. Mr. Green in 1878 remarked how faint it had appeared in the opposition of 1877 as compared with its aspect in former years. It would now seem to have regained its earlier distinctness, but not to present quite its original shape.

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* Comptes Rendus, Vol. xcviii. No. 13.

VARIABILITY OF SATURN'S RINGS *.—M. Trouvelot has recently published some interesting observations of Saturn's rings. In 1875 this observer announced, through the American Academy of Arts and Sciences, that the variable form of the shadow thrown by the ball of Saturn on the surface of the rings could only be caused by changes of level of the surface, that the nebulous ring had changed since the observations of Bond, Lassell, and others, and also that the Cassinian division was subject to variations in form. Subsequent systematic observations have confirmed M. Trouvelot in his opinion that the rings of Saturn are not fixed, but very variable. On Feb. 12th Encke's division was clearly seen, but on the 15th it had approached the Cassinian division, and later became invisible in an 8-in. telescope, its place being occupied by a larger and more strongly marked division much nearer the Cassinian division. On the same occasion the zone of ring A, situated between the above and the Cassinian division, was much whiter and brighter than at any previous observation. With respect to ring B, M. Trouvelot states that the three parallel zones which compose it had always appeared to him about the same width, that near the nebulous ring being the darkest, and the outer (that which forms the inner border of the Cassinian division) the brightest. During the evening of Feb. 15th the latter zone appeared narrower than usual towards the E. ansa, its width being certainly diminished by one-half. It was also brighter than usual, being very clearly distinguished from the intermediate greyish zone. The same phenomenon was observed on February 20th, but at the W. ansa, while it was very difficult to say if it existed at the E.

In 1882 very noticeable changes were observed on the inner zone which joins the nebulous ring, it being at times very dark and on other occasions scarcely perceptible.

The nebulous ring also shows very remarkable changes, being sometimes seen very easily towards the E., while the W. side is only seen with difficulty and at other times *vice versâ*; similar phenomena have been noted at the ansæ of the narrow bright zone on A between the new division of Encke and Cassini's.

Referring to the shadow thrown by the ball on the rings, M. Trouvelot considers it has changed the appearance presented in former years. The single curve concave towards the edge of the planet now appears as two, united at the interior end and forming a marked angle due to the intersection of the two curves. From these observations M. Trouvelot considers that the hypothesis of the rings being composed of a great number of satellites moving in independent orbits round the centre of gravity of the planet seems most probable, or at all events best explains the phenomena and easily explains why the period of rotation of the rings has not been determined; and he suggests that very delicate photometric observations taken near opposition and quadrature may throw some light on the composition of the rings.

* Comptes Rendus, Vol. xcviii. No. 16.

Uranus *.—MM. Perrotin and Thollon have continued the observations of the bright spot detected on Uranus on March 18 ('Observatory,' No. 85, p. 147) on the lower limb of the planet, and have ascertained that it is near the planet's equator. The spot is difficult to make out when on the disk, and when seen under such conditions its exact position is very uncertain. It becomes, however, more conspicuous as it approaches the limb; and it was thus observed with comparative ease on April 1, about 11^h, when it was at the northern extremity of the equatoreal diameter, and on the following day at about $10^h 40^m$ at the southern extremity. It was also seen again in this position on April 7 at $10^h 30^m$ and on April 12 at 11^h. The appearance of the phenomenon and the uncertainty in its duration, as seen on April 1, the night of best definition, showed that it was rather a luminous band than a simple spot. Allowing for this circumstance, the observations agree very fairly with a period of rotation of about ten hours. The appearance of the planet was sensibly the same as when first noted, except that the northern hemisphere was darker than the southern, and the dark spots compared previously to the spots on Mars resembled rather the bands on Jupiter. On April 12 M. Trépied was present and confirmed the previous observations, and noted a condensation in the bright part which had not been detected by the other observers. M. Perrotin appeals to observers possessing large telescopes to examine the planet, he being well aware of the possibility of illusions under the unfavourable conditions of observation.

Rotation of Mars †.—Mr. Denning has recently made a new determination of the period of rotation of Mars from observations of the Kaiser Sea, made 1869 Feb. 4 and 1884 Feb. 14, and obtains $24^h 37^m 22^s{\cdot}34$ for the time of a rotation. The mean of six of the best modern determinations gives $24^h 37^m 22^s{\cdot}626$, which must be a fairly close approximation to the truth.

Yale College Observatory.—The movement and pendulum of the gravity escapement clock Richard Bond, No. 367, have been erected, in a case designed by Dr. L. Waldo, to be used as mean-time standard in the extensive horological work of this observatory. The case is of cast-iron planed back and front, to which plate-glass doors are fastened, and rests on two brick piers rising level with the movement, to ensure stability in the pendulum suspension. The escapement and arc of vibration can be observed and adjusted very accurately. A barometer and thermometers are placed within the case, as also a cup of calcic acid, which can be exhausted to any barometric pressure by an air-pump attached to the side.

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* Comptes Rendus, Vol. xcviii. No. 16.
† 'Nature,' No. 759, 1884, May 15, p. 56.

MINOR PLANETS.—Dr. Luther, in a recent communication to us, points out that, including the newly discovered planet, which has received the name of Honoria, there are no fewer than 28 which have been observed during only one apparition. They are as follows:—

99 Dike.	183 Istria.	222 Lucia.
132 Æthra.	188 Menippe.	223 Rosa.
145 Adeona.	193 Ambrosia.	225 Henrietta.
149 Medusa.	197 Arete.	228 Agathe.
155 Scylla.	206 Hersilia.	232 Russia.
156 Xanthippe.	208 Lacrimosa.	233 Asterope.
157 Dejanira.	210 Isabella.	234 Barbara.
163 Erigone.	217 Eudora.	235 Carolina.
175 Andromache.	220 Stephania.	236 Honoria.
177 Irma.		

THE ASTRONOMICAL PRIZES OF THE ACADEMY OF SCIENCES.—The Lalande Prize has been decreed to MM. Bouquet de la Grye, de Bernardières, Courcelle-Seneuil, Fleuriais, Hatt, Perrotin, Bassot, Bigourdan, and Callandreau, the chiefs of the various French expeditions sent to observe the transit of Venus on December 6, 1882. The Valz Prize was awarded to M. Stephan, Director of the Marseilles Observatory, and discoverer of about 700 nebulæ, the positions of over 500 of which he has carefully determined.

THE governor of the province of Buenos Aires (M. Dardo Rocha) has decided to establish an observatory at La Plata, the new capital of the Province. 100,000 francs has been voted for the expenses of establishment, and 24,000 francs for the annual expenses. M. Beuf, formerly of the Toulon observatory, has been chosen Director. One of the first works is to be the construction of a map of the province.

A SMALL observatory is to be erected at St. John's College, Manitoba, for obtaining time. A transit instrument has been ordered from Messrs. Troughton and Simms, and Messrs. T. Cooke and Sons have just completed the construction of a mean solar clock for it. The clock has been presented as a memorial of Maria Margaret Macallum by her mother and sister.

MM. HENRY, of the Paris Observatory, have undertaken experiments with a view to applying photography to the determination of the distance and position-angle of double stars. Up to the present satisfactory results have been obtained on about 20 stars.

M. LEVEAU has been appointed to succeed the late M. Yvon Villarceau as Astronome Titulaire at the Paris Observatory.

PROF. H. GLYDÉN, Director of the Stockholm Observatory, has been appointed Professor of Astronomy at the Gottingen University.

Maxima and Minima of Variable Stars in 1884, June.

June 1 U Capricorni, M.
2 T Herculis, M.
2 S Vulpeculæ, M.
2·4 X Sagittarii, *m.*
2·8 U Sagittarii, M.
3 R Camelopardi, M.
3·4 W Sagittarii, *m.*
4 R Geminorum, M.
4 S Geminorum, M.
4·3 β Lyræ, *m.*
4·7 W Virginis, *m.*
5·3 X Sagittarii, M.
6·5 T Monocerotis, M.
6·6 W Sagittarii, M.
6·6 U Sagittarii, *m.*
9·4 X Sagittarii, *m.*
9·6 U Sagittarii, M.
10 T Sagittarii, M.
10 R Coronæ, M.
11·0 W Sagittarii, *m.*
12 S Leonis, M.
12·3 X Sagittarii, M.
12·9 W Virginis, M.
13 U Monocerotis, M.
13·4 U Sagittarii, *m.*
14·2 W Sagittarii, M.
16 T Aquarii, *m.*
June 16·3 U Sagittarii, M.
16·4 X Sagittarii, *m.*
17 L² Puppis, M.
17·2 β Lyræ, *m.*
18·6 W Sagittarii, *m.*
19 S Herculis, M.
19·3 X Sagittarii, M.
20·1 U Sagittarii, *m.*
21 T Urs. Maj., *m.*
21·7 W Sagittarii, M.
22 R Lyræ, *m.*
22·0 W Virginis, *m.*
23·1 U Sagittarii, M.
23·5 X Sagittarii, *m.*
25·6 T Monocerotis, *m.*
26 S Aquarii, M.
26·2 W Sagittarii, *m.*
26·3 X Sagittarii, M.
26·9 U Sagittarii, *m.*
27 R Virginis, *m.*
27 T Canis Min., M.
29·3 W Sagittarii, M.
29·8 U Sagittarii, M.
30·1 β Lyræ, *m.*
30·2 W Virginis, M.
30·5 X Sagittarii, *m.*

M, signifies maximum; *m*, minimum.

Variables of Short Period.

δ Cephei.	η Aquilæ.	U Coronæ, *m.*	S Cancri, *m.*
d.	d.	h m	h m
June 1·85, M.	June 2·2, M.	June 4 16 13	June 9 14 23
5·55, *m.*	7·0, *m.*	11 13 55	28 13 38
7·15, M.	9·4, M.	18 11 38	U Cephei, *m.*
10·95, *m.*	14·2, *m.*	25 9 20	h m
12·55, M.	16·6, M.	δ Libræ, *m.*	June 5 16 56
16·35, *m.*	21·4, *m.*	h m	10 16 36
17·95, M.	23·7, M.	June 2 10 20	15 16 15
21·65, *m.*	28·5, *m.*	9 9 54	20 15 55
23·25, M.	30·9, M.	16 9 28	25 15 34
27·05, *m.*		23 9 2	30 15 14
28·65, M.		25 16 53	
		30 8 36	

Astronomical Memoranda, 1884, *June.*

Equation of Time:—Sun *before* Clock, June 1, 2^m 22^s; June 11, 0^m 34^s; Sun *after* Clock, June 21, 1^m 34^s; June 30, 3^m 27^s.
Sidereal Time at Mean Noon:—June 1, 4^h 41^m 30^s; June 11, 5^h 20^m 55^s; June 21, 6^h 0^m 21^s; June 30, 6^h 35^m 50^s.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
May 31	15 49	8 4	344 28	−0 27	296 7
June 5	15 47	8 9	346 25	+0 9	229 57
10	15 45	8 13	348 27	0 45	163 46
15	15 44	8 16	350 35	1 21	97 35
20	15 45	8 18	352 47	1 56	31 24
25	15 46	8 19	355 2	2 30	325 12
30	15 49	8 18	357 19	+3 4	259 1

The position-angle of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon.

Moon.	*sets.* h m		*rises.* h m		*rises.* h m
June 1	12 57	June 11	9 51	June 21	15 19
2	13 20	12	10 24	22	*sets.*
3	13 44	13	10 53	23	8 12
4	14 10	14	11 19	24	8 59
5	14 39	15	11 45	25	9 37
6	15 11	16	12 11	26	10 9
7	15 49	17	12 38	27	10 36
8	*rises*	18	13 8	28	11 1
9	8 31	19	13 44	29	11 25
10	9 14	20	14 27	30	11 49

Full Moon, June 8, 7^h 49^m; Last Quarter, June 16, 2^h 34^m; New Moon, June 22, 17^h 33^m; First Quarter, June 29, 18^h 15^m.

Mercury is an evening star, at greatest elongation (23° 3′ W.) June 12^d 15^h.

Venus is an evening star, stationary among the stars June 19^d 2^h. Diameter:—June 1, 34″·8; June 30, 53″·6. Illuminated portion of disk 0·177 on June 15.

June 1, R.A. 7^h $37^m{\cdot}7$, Dec. 24° 9′ N., tr. 2^h 56^m, sets 11^h 18^m
30, 7 51 ·7, 18 43 N., 1 15 8 59

Mars in Leo, in conjunction with the Moon June 27^d 20^h. Diameter:—June 1, 7″·2; June 30, 6″·3. Illuminated portion of disk 0·904 on June 15.

June 1, R.A. 10^h $3^m{\cdot}8$, Dec. 13° 20′ N., tr. 5^h 22^m, sets 12^h 35^m
30, 11 1 ·9, 7 8 N., 4 26 11 5

Jupiter in Cancer; in conjunction with the Moon June $25^d\ 4^h$. Diameter:—June 1, 31″·4; June 30, 29″·8.

June 1, R.A. $8^h\ 16^m·6$,	Dec. 20° 22′ N.,	tr. $3^h\ 34^m$,	sets $11^h\ 30^m$
30, 8 39 ·7,	19 3 N.,	2 3	9 51

Saturn is in conjunction with the Sun June $3^d\ 9^h$.

Uranus in Virgo; in Quadrature with the Sun June $14^d\ 8^h$.

June 1, R.A. $11^h\ 39^m·2$,	Dec. 3° 5′ N.,	tr. $6^h\ 56^m$,	sets $13^h\ 16^m$
30, 11 40 ·6,	2 54 N.,	5 4	11 22

Phenomena.

	G. M. T. h m			G. M. T. h m	
June 1	10 20	J. ii. Tr. I.	June 27	10 27	34 Sextantis Oc. D. 55°.
3	9 45	J. ii. Ec. R.			
10	9 41	J. i. Tr. E.			

The angle is reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

ERRATUM IN No. 85.

Page 139, line 27, *for* latter *read* former.

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THE OBSERVATORY,

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No. 87. JULY 1. 1884.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1884, June 13.

E. Dunkin, F.R.S., *President*, in the Chair.
Secretaries: E. B. Knobel, and
Lieut.-Col. G. L. Tupman.

The Minutes of the last Meeting were read and confirmed.

Col. Tupman announced that 71 presents have been received since the last Meeting. Those calling for special notice are:—the first volume of a republication of Dembowski's double-star observations by the Accademia del Lincei of Rome, to be published in two volumes; and a copy of Sherburne's poetical translation of Marcus Manilius, 1675, presented by Mr. C. L. Prince, of Crowborough.

Mr. Knobel. The volume which Mr. Prince has presented is a great rarity. For six years I have searched all the booksellers' catalogues without finding it, and Mr. Quaritch has also been looking for it for me. The volume is valuable as an English translation of Manilius's 'Astronomicon Poeticon,' and also for the extensive list of Oriental astronomers it contains.

The President. I may say, in regard to this book, that four or five months ago a gentleman called at the Greenwich Observatory offering a copy for sale to the Astronomer Royal, and it was purchased for the library of the Observatory. It is remarkable that two copies of so rare a work should come to light almost at the same time.

Mr. Newall. I think there is an engraving of the Sun in this book which shows the rice-grains. It looks like a rice-pudding with a number of raisins in it.

The thanks of the Society were unanimously voted to the respective donors.

Mr. Green exhibited three drawings of Saturn. He said,—There seemed to be some doubt as to what he had expressed at a former meeting in regard to the existence of certain marks on Saturn, when a drawing was exhibited by Mr. Pratt, of Brighton; and there was some misunderstanding between himself and Mr. Ranyard in regard to a certain new belt. He now exhibited a drawing made at the close of 1883, when the ball of Saturn was projected above the ring, and in it there was no evidence of a small sharp white belt which appeared in a drawing made during the last opposition. He also had a drawing which had been made from the formula given in the 'Nautical Almanac.' This he had brought with him to a previous meeting, but, on comparing it with the beautiful drawing by Mr. De La Rue, thought he (Mr. Green) had made a mistake in the size of the ball and did not exhibit it; but on examining it at home he found that it agreed with the figures in the 'Nautical Almanac,' and therefore these proportions could not be right, as they made the ball too small for the ring. Another drawing he had made by eye estimation was by no means so large in the quantity of the ball of the planet as those generally seen in illustrations of Saturn.

Mr. Ranyard. Do you make the ball smaller than in the 'Nautical Almanac'?

Mr. Green. If I take the Almanac the ball becomes too small.

Mr. Ranyard. And the space too large between the ball and the rings?

Mr. Green. Yes.

Mr. Knobel asked whether Mr. Green had seen some recent measures by Mayer and Otto Struve, which were most valuable.

Mr. Green said he had not. He did not suspect the values in the 'Nautical Almanac' until he discovered that the ball would be too small with the numbers given in the Almanac.

Mr. Ranyard. I think these drawings nearly correspond with what I have seen; there was a slight division between my dark belt and the cap of cloud which extended to the southern pole of the planet. It is the same dark belt which Mr. Green, I think, shows with a division between it and the cap.

Mr. Green. Your dark belt was a broad one, and it is equal now because they have developed a little.

Mr. Knobel read a paper by *Professor Newcomb*, entitled "Notes on Mr. Stone's explanation of the Errors of Hansen's Lunar Tables." Mr. Stone had maintained, said Prof. Newcomb, that no one had answered his argument. To answer it one must first understand it, and that was more than he had been able to do. He had been entirely unable to master the reasons and equations given on page 336 of vol. 43 and page 226 of vol. 44 of the 'Monthly Notices.' The value of the argument depended largely on the calculations. Mr. Stone had not attempted to make out a logical case. He understood Mr. Stone's conclusion to be that the increasing errors of Hansen's tables since 1864 arise from the substitution of Le Verrier's tables for Carlini's in the British Nautical Almanac for 1864 and subsequent years. The logical process for establishing this would be (A) to discover what the apparent errors of the Lunar tables would have been had Carlini's solar tables been continued in use to the present time; (B) to show what the errors are now with Le Verrier's tables; and (C) to determine what theoretical errors have arisen from the cause Mr. Stone had discovered. But

Mr. Stone had only dealt with B, and did not show what the difference would be, except by the unknown symbol *t*. Not only had Mr. Stone those two fatal defects in his argument, but he had resisted all appeals to give a general definition of the symbol *t*. To apply Mr. Stone's theory in the most natural way, we should count the time *t* from 1728, when the two tables agreed in mean longitude. That would result in a difference of more than 2′, whereas the errors of the tables were scarcely one tenth of that amount. Mr. Stone counted from 1864. But Hansen's solar tables were introduced into the 'American Ephemeris' in 1858, so that in 1864 there should be, according to Mr. Stone's formula, a difference of 4″·5 in the Moon's place between it and the 'Nautical Almanac.' The numerical results of the two ephemerides were, however, substantially identical, so that the formula must be erroneous.

Mr. Stone. Of course I quite understand that the general feeling among astronomers is rather against the view which I have brought forward; but still I certainly have not seen any valid objection to my theory, and the facts agree with my views, which are represented very fairly in this chart ["a table showing the excess of the longitude of the Moon from Hansen's tables from observations showing the change in the unit of mean time made in 1864"]. With regard to the statement of Prof. Newcomb that I have only given the errors of Hansen's tables with the mean times reduced on Le Verrier's scale, I must say, in justification of myself, that that is not entirely due to me. I sent in a large number of calculations and facts to the Society, containing a comparison between every observation of the Moon that had been made from 1864 to 1881 on the Greenwich meridian as compared with Hansen's tables with the mean times as computed in the usual manner and as compared with observations when the times were reduced to one uniform scale by the corrections of my theory to reduce them to Carlini's scale of time; and the corrected and uncorrected results are shown in this diagram. If that paper has not been printed, the fault is not mine. The chart here exhibited is, I think, a satisfactory answer to the greater part of Prof. Newcomb's objections. Without entering into any theory at all this chart shows, and it is obvious to any one, that the line of mean vibration or the line above and below which the periodic errors rise and fall has continued sensibly unaltered during 110 years, but that it changed its direction about 1864. For one, I am prepared to say I do not think that any computation of long inequalities can possibly reconcile such a change as this. Now comes the question, why do I think of the year 1864? Because, in the discussion of the Greenwich observations, we made a difference in our solar tables in that year and altered the mean times. If I had been trying to fix empirically the point of departure, I should not have fixed on 1864, but 1862, and perhaps even 1860 or 1861; but I went to 1864 because there was a difference made in our determination of time. With regard to my not answering Prof. Newcomb's statement about Carlini's tables, I was perfectly able to answer it, but I did not like the way in which it was put. Newcomb merely tried to prove that my views were impossible, and that this could not be an explanation of the observed facts, by the simple statement that as Hansen's tables gave the position of the Moon in Greenwich mean time, nothing I did afterwards could affect the agreement between Hansen's tables and the observations, not even apparently an alteration of the scale of my mean times. This appeared to me so curious a way of treating a serious problem that it called for no answer, and so I left it alone. In regard to 1858, if the Washington observers have been using, since 1858, Hansen's solar tables, in which the mean motion in longitude of the Sun does differ from that formerly used, then we should expect the difference H—O would run away from 1858 as from 1864; but the only difference would be between 1858 and 1864, and this is only a question of 4 seconds of arc altogether. I had not a sufficient number of Washington observations to form an opinion on that point or to compare with the Greenwich observations. But as the Greenwich observations from 1858 to 1861 show a depression, the first 3″ would simply be used in concealing this depression. But whether my views are right or wrong, Prof. Newcomb has not met in the slightest degree the serious point which I have been raising. The question is a physical question, and is not a question of mere numbers considered without regard to the physical facts they stand for; and there is one point which indicates the nature of the question. We have all been in the habit of saying that when we replaced Bessel's tables by those of Le Verrier we changed our estimation of the tropical year by $1^s{\cdot}46$ per annum. The tropical year bears a certain ratio to that of the sidereal day, namely that of the angular velocity of the Earth about its axis to the Sun's mean motion in longitude. We have been in the habit of saying that we have been using in all cases the true mean solar day; and although the tropical year and the sidereal day are physical facts, we have apparently seen no difficulty in accepting different estimations of the tropical year. By no possibility can changes occur if our time measures were placed on a proper physical basis. The difference of estimation of $1^s{\cdot}46$ per annum for the tropical year indicates exactly the change required to destroy the errors in the lunar tables, and yet we are asked to believe that this is an accidental coincidence. I was not led by any empirical curve to assume the change in the adopted value of the unit of time, but by the necessity of keeping a constant ratio between the tropical year and the sidereal day, and between the angular velocity of the Earth on its axis and the Sun's mean motion in longitude. The view that I have brought forward simply assumes the continuity of our measure of time. My theory is that the relative position of the meridian and the mean Sun and the mean noon must not arbitrarily be changed. If you do this you disarrange the whole of the existing tables. I have thus been able to account, at all events to my own satisfaction, for the existence of the only *empirical* long inequality in Hansen's tables of the Moon. It is the long inequality depending upon the action of Venus on the Earth; and in my opinion the necessity of the introduction of such an inequality which was found necessary by Hansen to completely represent the Greenwich lunar observations can be explained, on the general principle of forced vibrations, from the fact that the absolute epochs fixed by Bessel from Bradley's observations about 1750, and from his own observations about 1820, for the determination of his precession constants, were fixed without allowing corrections for the long inequality due to the action of Venus discovered by Airy in the theoretical expression of the Sun's mean longitude, and are therefore inconsistent with each other; and this strained condition amongst the different epochs would be continued until fresh determinations shall have been made with solar tables freed from this imperfection. An inequality thus indirectly introduced would have the same period as the original inequality, but not the same epoch, and, in accordance with my views, its coefficient would be between 18″ and 25″. It appears to me that this is the true explanation of the necessity for the introduction of Hansen's empirical inequality with the same period as Airy's inequality. Also the same theory accounts for the missing four seconds of secular acceleration in the Moon's mean motion. The real point at issue between me and those who differ from me is whether the term of the form $24^h \cdot \frac{\delta n}{n} \cdot t'$, which comes into the expression of the sidereal time at mean noon, can or cannot be neglected when we change the adopted value of the Sun's mean motion from n to $n+\delta n$. Professor Newcomb has not and no one yet has attempted to deal with this, which is the only important point. If the English and American observers all commit the same mistake and make the same error at the same time, their sidereal times at mean noon will agree, but be equally in error; their mean times, therefore, will be equally erroneous, and the tabular places brought up for the time of observation will be equally discordant with the observed places.

I think I have met Prof. Newcomb's objections pretty straightforwardly. I cannot enter into the whole of this matter. To understand the equations which I have given two things are required—that I should explain myself with some clearness, and that there should be an effort on Prof. Newcomb's part to understand me. Instead of there being any reasonable doubt about the epoch from which my t is measured in the 'Monthly Notices' for May 1883, I have said that the unit of time was changed in 1864; and if you change the unit of time you must either correct your times to the old scale or apply a correction to the Moon's mean longitude, which, as due to the error in time, commences at the same instant as the error in time commences and lasts only as long as the error lasts.

I have actually broken off the tabular results with the year 1863 and stated "here the change of unit took place."

Mr. Knobel. What is the deviation between 1858 and 1864 as compared with the American Ephemeris?

Mr. Stone. These deviations are only 3 or 4 seconds of arc and are disguised by the negative errors of Hansen's Tables about that time.

The Astronomer Royal. This is not a question of a difference between observation and computation, but of a difference between two computations. It is a question why there is not a difference between the computations of the American Ephemeris and of the 'Nautical Almanac.'

Mr. Stone. Because in each case the same mistake has been made. The term $24^h \cdot \frac{\delta n}{n} \cdot t'$ has been neglected in both cases.

The Astronomer Royal. That is not an explanation of it, for in the 'Nautical Almanac,' up to 1864, no change had been made in the adopted value of the Sun's motion, so that $\delta n = 0$ in that case.

Mr. Gill. Why are the numerical results nearly the same?

Mr. Stone. Simply because in making the change we have disturbed the relation between the meridian and the mean Sun; and assuming that there was no additional term required, by its neglect you get only $\frac{1}{365}$th part of the true difference.

The Astronomer Royal. The point is this, that two different computers start from 1858 and 1864 respectively and get similar results, whereas, according to Mr. Stone, they ought to get results differing by about $4\frac{1}{2}$ seconds. Why is there not this difference? That is the point Professor Newcomb puts.

Mr. Stone. It is simply the question of the neglect of the term $24^h \cdot \frac{\delta n}{n} \cdot t'$.

The Astronomer Royal. We have this fact, that from 1858 the American Ephemeris adopted a new value of the motion of the Sun, which is practically the same as Le Verrier's. Up to the beginning of 1864 the English 'Nautical Almanac' used Bessel's. What we want to know is whether the English 'Nautical Almanac' was right at the end of 1863, or the American Ephemeris, because the two agree at that time in the places of the Moon, though adopting different values of the Sun's motion. One or other should be wrong according to Mr. Stone's view.

Mr. Stone. When you speak of right or wrong, it is a question whether you neglect this term $24^h \cdot \frac{\delta n}{n} \cdot t'$ or not. If you neglect it, the separation of the two expressions is exceedingly slow. Prof. Newcomb compares the numerical values without taking this term into account. The whole difference between me and those who differ from me is whether this term is absolutely required or not when n is changed in $n + \delta n$ and we determine our mean times from the sidereal times at mean noon, or through these sidereal times at mean noon refer the right ascensions and north-polar distances derived directly from the tables to the meridian for comparison with observation. If this term is required to secure continuity in our measures of time, I must be right; if it is not required then I must be wrong, for you will get a different right ascension and north-polar distance on the meridian according as you reject or retain this term.

Colonel Tupman. I have a paper, sent by Dr. Copeland, suggesting improvements in the transit-circle. There are two main features which he desires me to point out. He proposes to have two object-glasses of precisely equal focal length, one at each end of the main tube. Fiducial lines are engraved on the exterior surfaces. There is supposed to be no difficulty in making these lenses of equal focal length. This plan enables the telescope to be reversed end for end. The next feature is that the eyepiece or apparatus for examining the fiducial lines, instead of being attached to the tube, is attached to a framework revolving on the axis. This eyepiece can be turned over to either end, and exerts no weight on the tube. There is also the flotation in mercury, as suggested by Mr. Common.

Mr. Ranyard. Where is the eyepiece?

Colonel Tupman. It is supported by the revolving frame. The lines are engraved on the external surfaces of the object-glasses. The external surface of one object-glass is exactly in the focus of the other object-glass, these being exactly equidistant from the horizontal axis.

Mr. Gill. There are many constants in a transit-circle which could be eliminated by two object-glasses, to be used alternately, one at the eye end, and the other at the opposite end; but it is a question whether such a thing is practicable in a real instrument. I somewhat doubt it unless you could introduce a lens in the form of a Barlow-lens which could be moved in the tube; but that would introduce errors almost greater than at present. We have overlooked the old form, introduced by Struve and Repsold, of the interchangeable object-glass and the eye end, which in most forms of instrument will eliminate nearly the whole of the systematic errors connected with flexure, if capable of reversal. It has been said that if an instrument is reversed it becomes a new instrument. There is one sense in which that is true; if the bearings or the pivots are slightly different, you may have different errors in the two positions. But if the pivots A and B rest on bearings A′ and B′, and if you now simply reverse the instrument so that the pivot A rests on the bearing B′, and the pivot B on the bearing A′, I defy any one to prove or show the remotest grounds for suspecting that the absolute flexures are changed, or that any change is produced except possibly minute changes in the errors of the axis of rotation, and these can be very easily determined. I think it is capable of proof that with interchange of object-glass and eye end, and with reversion of the pivots on the bearings, every conceivable instrumental error can be eliminated from the observations, so far as lateral or vertical flexure is concerned. But that is a question which can be settled both practically and theoretically.

The President. Mr. Gill, I believe, has an interesting communication to make.

Mr. Gill. The communication I have to make is simply in continuation of a statement I made at the March meeting, that I had prepared a scheme for the investigation of the parallax of stars, but that the carrying it out in the southern hemisphere depended on the generosity of the Lords Commissioners of the Admiralty in providing me with a heliometer necessary for the purpose. I have this afternoon had an interview with the authorities of Her Majesty's Treasury, and am permitted to state that they will not be wanting in the necessary generosity. (Applause.)

Mr. Ranyard read a paper entitled "Note on the Cause of the Blurred Patches in Instantaneous Photographs of the Sun." He said:—"The beautiful photographs of the Sun taken at Meudon by M. Janssen exhibit curious patches of a lighter shade within which the definition of the rice-grain structure of the photosphere is interfered with. M. Janssen is inclined to attribute these patches to solar clouds or gaseous masses above the photosphere, which he supposes change very rapidly. I have recently been making some experiments which show that similar indistinct lighter patches on instantaneous photographs of the Sun may be caused by currents of heated air. The experiments have been made with a reflecting telescope with a heavy iron tube. If the image of a bright star is observed out of focus on any ordinary night, ripples of light may be seen passing across the bright disk, which is really the image of the speculum with the flat projected on its centre. That these ripples are due to the unequal refraction of heated-air currents may be proved by placing a heated body in the tube of the telescope. I made use of a hot iron chisel, which I placed in different parts of the mouth of the telescope, and let down the tube by a string. This greatly increased the distinctness of the ripples, as well as the velocity with which they passed across the image. The dark lines corresponding to the supports of the flat enabled one to localize the part of the tube from which the heated air was rising and showed that it corresponded with the place where the hot piece of iron was held. Though these ripples are only seen when the star is out of focus, they must affect the sharpness of the image in focus, and cause scattered light around it. In the image of a uniform bright disk their effect would evidently be to give rise to areas of greater and less brightness, which would float across the field as the heated air rises in front of the telescope. In order to test this, I took some instantaneous photographs of the Sun with a piece of heated iron in the mouth of the telescope, and also when the Sun was seen just above a heated roof. In both cases the limb of the Sun is much distorted, and brighter areas are seen extending on to the disk. In the case of the photograph taken with the hot iron in the mouth of the telescope, the brighter areas have a tree-like form, and they appear to have some connection with the irregularities of the limb." Mr. Ranyard exhibited some photographs to the Meeting; they were taken in a camera seven feet six inches long, attached to the side of an eighteen-inch telescope of nine feet focal length. The light is reflected from the flat of the telescope as in the ordinary Newtonian form, and again reflected by a second flat into the camera, which is provided with a Dallmeyer lens, giving an image of the Sun about thirty inches in diameter.

Mr. Common. I did not hear exactly how Mr. Ranyard got these photographs. The important part is the exact width of the shutter.

Mr. Ranyard. The width was three eighths of an inch. I cannot estimate with great exactness the time of exposure. Janssen estimates it at $\frac{1}{1500}$ of a second; I think mine was not $\frac{1}{50}$ of a second.

Mr. Common. The state of the air and sky &c. will tell in this work, but I expected Mr. Ranyard used very much less than three eighths of an inch. With a fine aperture in the shutter you get diffraction and no image, but if you have a wide opening, you get rid of atmospheric and light disturbances. I cannot account for Mr. Ranyard's images being so bad by any instrumental means he has used. It must be either the sky or the telescope that was in fault; and if not the telescope, the magnifying arrangement.

Mr. Ranyard. It was by placing the hot iron in the tube and taking the image above it.

Mr. Common. I have no doubt with perseverance you can get bad photographs; but the important question is how to get good ones.

Mr. Ranyard. A great help is to know the cause of particular failure.

Mr. Newall. I have made spots on the Sun a study; I began with a 2-inch object-glass, and in three years made upwards of 100 sketches. I have heard of sudden movements in the solar spots, but the only sudden movement I ever observed was when I saw a spot suddenly divide into two. I had a little patience to watch, and the spot came back again to its original shape. A sharp thin cloud had cut the spot in two, and when the cloud passed away it resumed its original form. Perhaps that is the cause of a good many of these supposed rapid movements of solar spots.

The Rev. Mr. Howlett. It is now nearly a quarter of a century since I first began to draw sun-spots. When one thinks of the splendid instruments now being used by some Fellows of this Society, I am almost ashamed to speak of my 3-inch achromatic, but it admits abundance of light, and there is no question that optical appliances on a small scale are trustworthy. My time for looking at the Sun was generally early in the morning, when the disturbing effects of currents of the atmosphere and its different refracting powers and properties referred to by Mr. Ranyard are at a minimum. When the Sun is in a general state of tremor, this distortion is, to some extent, communicated to every feature, and therefore when you are looking carefully at the Sun you can often only depict at interrupted moments the real state of the disk. The method I pursued was similar to that of Father Perry. I projected the Sun's image upon a screen, so as to obtain a disk of about 32 inches in diameter, or about 1 inch to a minute of arc. I make the drawings on a plate of ground-glass, and flood it with Canada balsam to restore the transparency. I propose to show you some of the most characteristic spots which have appeared during the last 25 years. What the veiled spots which Father Perry referred to at the last Meeting are, I cannot imagine. I am sure that hundreds and thousands of times, when gazing at the image of the Sun, I have seen all the granulations and striæ and other delicate features, but nothing like veiled spots. [Drawings of several spots were shown, magnified on a screen by lime-light, Mr. Howlett mentioning the chief points of interest.] Referring to the faculæ, Mr. Howlett said they are fairly stationary. He supposes the reason we do not see them in the centre of the disk is because they are not intrinsically much brighter than the photosphere, but when they draw towards the limb of the Sun they become visible. Whether they are clouds or mountains of light, their summits may suffer less absorption of light by the solar atmosphere than the plains out of which they rise, and that may be the reason why we see them so distinctly near the Sun's limb. Mr. Howlett remarked that he had no further theories to propose, but he could mention hundreds of other remarkable changes that might not be unworthy of the attention of this Society. [Thanks were voted to Mr. Howlett for his explanations and exhibition of his drawings.]

The following papers were also announced:—

G. M. Seabroke. "Fourth Catalogue of Micrometrical Measures of Double Stars made at the Temple Observatory, Rugby."

J. E. Gore. "On a new Star in Monoceros."

R. S. Newall. "The Nebula in Orion."

Dr. J. Morrison. "The Apparent Orbit of a Satellite of a superior Planet."

H. Pratt. "The Physical Features of Saturn."

A. Marth. "Ephemeris of the Satellite of Neptune, 1884–85."

A. Marth. "Ephemerides of the Satellites of Saturn, 1884–85."

E. J. Stone. "On Professor Newcomb's Empirical Corrections as a means of Restoring an Agreement between Theory and Observation in the case of the Moon."

A. A. Common. "Faint Stars near Alcyone, and near β^1 and β^2 Capricorni."

The following gentlemen were elected Fellows of the Society:— Mr. Robert Stuart Callcott, and Samuel Johnson, M.B.

The Meeting adjourned at $10^h\ 10^m$ P.M.

The Micrometer.

It is a well-worn adage that "science is measurement," but of no science is this so true as of astronomy, and a complete history of the gradual development of astronomical measuring instruments, and of the increased accuracy of the observations made by their means, would fall little short of being a history of modern astronomy itself. The subject therefore of the construction and use of micrometers is one of the most important on which a practical astronomer can treat. It is gratifying therefore to find that the preparation of the article on the Micrometer, in the edition of the 'Encyclopædia Britannica' now in course of issue, has been placed in hands so thoroughly able and practised as those of Dr. Gill.

Dr. Gill does not spend much time or space on the history of the micrometer; and, indeed, no very useful purpose would have been served by an account of the long series of minute alterations which have finally resulted in the perfected instruments we now possess. He gives, however, an account of the earliest form of micrometer, which we owe to the genius of Gascoigne, the friend of Crabtree and Horrox, and which in his hands proved capable of results of very considerable accuracy, and refers briefly to the micrometers of Huygens, Auzout, and Picard, and to the introduction of spider-webs in place of metallic wires; but beyond this the gradual evolution of the modern forms of micrometer is not traced out in detail, Dr. Gill preferring to give a careful description of those forms which practical experience has shown to be best adapted for their work. These descriptions are rendered clearer and more connected by the classification of the modern filar micrometer under five types, one or more instruments being fully discussed under each type. Of the five types, the first is the ordinary English form with two independently moving webs; this form is due to Troughton, and is described in detail with improvements since made in it by Simms, Cooke, and others. These consist of the introduction of two springs to each slide, in order to attain symmetry and avoid "loss of time," and in an alteration in the mode of communicating motion to the slides. Dr. Gill criticizes the method of giving the screw a spherical bearing resting in a hollow cone as demanding "an almost inconceivable accuracy of construction," and prefers the method adopted in Repsold's and the Lindsay-Gill reading micrometers, where the screw passes through cylindrical holes and its end bears upon a firm base plate. A slight confusion is introduced into the description of the latter instrument by the repetition in the diagram of the same letter *a* in two entirely different places. The second type, the ordinary German form, differs from the first in that one of the webs is moved by a coarse screw which has no divided head, and the Merz micrometer of the Cape Observatory, made on Fraunhofer's model, is described in illustration of this form. The third type embraces micrometers with only one screw; reading micrometers are therefore described under this heading. The fourth type possesses an important advantage over the others in having a screw head, by which a fine movement can be given to the whole micrometer box in the direction of the axis of the micrometer screw. The micrometer constructed by Repsold for the Cape Observatory is very fully described in illustration of this type, and it certainly would seem to deserve the very high praise bestowed upon it, for every regard seems to have been paid to the convenience of the observer, and the workmanship is of very high character. With the highest powers "the webs can be brought into apparent contact with such precision and delicacy that the uncertainty of measurement seems to lie as much in the estimation of the fraction of the division of the head, as in the accuracy of the contact." The fifth type consists of micrometers with two eye-pieces, for measuring larger angular distances than can be effected by the ordinary forms. Of these, the best is Prof. Pritchard's ingenious "duplex" micrometer by Grubb, and a brief but sufficient description of it is given. Dr. Gill, however, is clear that "Prof. Pritchard claims too much" for it "when he estimates its work as equal in accuracy with that of the heliometer." It has, however, certainly done good work.

The most considerable portion of the article is taken up with the heliometer, the history and description of which takes more space than all the varied forms of micrometers put together. A captious critic might object that the heliometer is scarcely a micrometer in the sense in which the word is defined in the opening words of the article, that it is already sufficiently elaborated to rank as a distinct instrument, and that the information here given might have more fittingly formed the subject of a separate paper. This portion of the article is, however, perhaps even more valuable than the rest, as it is a careful description of an instrument of precision, which is destined to a wider and more general employment than has fallen to it at present, by an observer in whose skilful hands it has already performed work of the very highest importance. Bessel's heliometer, that of the Radcliffe Observatory, those ordered for the Russian Transit of Venus expeditions in 1874, and the new one for Yale College, New Haven, U.S., are described at length. On the other hand, not much attention is given to the converse of the heliometer, the double-image micrometers with divided lenses, the Airy micrometer especially being somewhat slurred over. The various micrometers which involve the employment of the diurnal motion are briefly touched upon, this section ending with a well-merited tribute of admiration to Prof. Peters, who has planned to map all stars down to the 14th magnitude between declination $+30°$ and $-30°$, and who has already published 20 charts of this series in which all the stars down to the 11th magnitude have been actually observed, and those down to the 14th filled in by alignment; and when "*all this results from the unaided labour of a single observer*, we find that our ideas of *the possible* have to be modified, when such a man undertakes a work with persistent unity of purpose for more than twenty years."

A notice of Burton and Grubb's ghost micrometer, to which too little attention has hitherto been paid by observers, but which may ultimately supersede the filar micrometer, concludes this very able and complete account of the different orders of astronomical micrometers.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Shadows cast by Faculæ.

SIR,—

It may be worth while, in connection with the curious observation of M. Trouvelot, and the confirmation of it I think I obtained (*vide* 'Observatory,' No. 86, p. 170), to call the attention of your readers to a few facts bearing on the possibility of the existence of such shadows as those supposed to be seen by us, facts which even seem to render their occurrence very probable. The theories of sun-spots are so conflicting that anything tending to remove the confusion is very welcome, and should not be condemned as *à priori* impossible, on the ground of any existing theory.

It is clear that one condition of the existence of a true shadow cast by the upper layer of the photosphere on the penumbra of a spot is that the inner side of the upper layer should radiate less light than the outer side; in other words, that the most brilliant light is produced just on the surface of the Sun's globe. Now there are considerations which at least favour this view. The penumbra, which is, usually at least, beneath the general surface level, is less bright than the surface, the umbra and nucleus at greater depths much darker. This is usually attributed, and is no doubt partly due, to masses of absorbing gases in the spot. But that, I think, can hardly fully account for it, as the spectra of the penumbra and umbra show *general* as well as selective absorption. Now if this general absorption were caused by the gases of the Sun's atmosphere it ought to cause not only a darkening, but a *reddening* of the penumbra, and the nucleus especially would be dark red in comparison to the general body of the solar surface. The limb of the Sun is thus reddened by absorption of the violet part of the spectrum, according to Prof. Langley's observations. Now the penumbra is not reddish, but has always appeared to me more pure in its *whiteness* than the surface, though radiating less light.

Moreover the penumbra is not always *below* the photospheric surface. It certainly is in most cases; but I have seen spots which near the limb looked more like elevations than depressions, and the observations referred to in Young's treatise on the Sun can hardly be looked upon as decisive of *all* cases. The penumbra at all events is sometimes much less depressed than at others, and does not appear greatly or at all brighter at such times. These considerations point to a less *intrinsic* brightness for the penumbra than for the surface. Moreover there are many reasons known to astronomers for our believing that the mass of the Sun below the surface is less fitted by its condition to radiate than at the surface.

If the penumbra be less bright, it will be partly illuminated by a fierce reflection from above, as well as directly by lofty faculæ round the edge of the spot; an overhanging edge will then cut off this light and cast a shadow. Herschel's old theory of the spots was deduced by one who was a keen observer, and was suggested directly by the appearances observed; only on his assumptions the penumbra should have been equally bright, or brighter at the edge of the spot, while the opposite is the case in fact. But his ideas of a darker (*i. e.* less radiating) inner layer, capable of reflection of the light from the surface and brilliant atmosphere above, deserve consideration in view of M. Trouvelot's observation, which, from your account, he seems to have been almost forced to interpret as a *shadow* in the *Sun*. The circumstances required by the explanation I have indicated were also present, as lofty faculæ overhanging and surrounding the spot *close to its edge* are shown in his drawing as in mine; and I, at least, had no thought when making it of connecting this with the probability of the formation of a shadow.

Barrhead, Scotland,
1884, May.

Yours faithfully,
EDWARD B. KIRK.

The Eclipse of Pericles.

SIR,—

In his excellent and useful little book, 'Eclipses Past and Future,' the Rev. S. J. Johnson refers amongst others to the solar eclipse which occurred in the first year of the Peloponnesian war, and in reference to which Plutarch tells us that Pericles endeavoured by an illustration to remove the superstitious fears of the pilot of his boat. The date generally accepted for this event is B.C. 431; and an eclipse of the Sun occurred on the 3rd of August in that year, which is usually supposed to be the one in question. Mr. Johnson, however, on the ground that this eclipse was more partial than had been thought (the obscuration not exceeding seven tenths of the Sun's diameter), proposes to substitute for it that of March 30, B.C. 433, thus carrying back the outbreak of the Peloponnesian war to a date two years earlier. Now without entering into a dispute about the exact season of the year implied in the word θέρος, which Mr. Johnson thinks may in that country have been taken to include a time as early as the end of March, I would submit that it is quite impossible that all the events of the campaign as narrated by Thucydides could have occurred before that time. We have the attempt of the Thebans to surprise Platæa and its failure; the march of the Spartan army into Bœotia under King Archidamus; the long and ineffectual siege of Œnoe; the slow march into Attica; the long encampment at Acharnæ; the final retreat of Archidamus, on finding that he could not succeed in drawing the Athenians, restrained by Pericles, into an engagement. It was only after the retreat had actually begun that the latter fitted out a naval force to ravage the coasts of Peloponnesus; and it was at the embarkation of this that Pericles, according to Plutarch, held up his cloak to re-assure the pilot, frightened by the eclipse, and told him the only difference between that and the phenomenon was that something larger than his cloak (he does not seem to have explained what) caused the obscuration of the Sun. Now all these events could not have taken place before the 30th of March, whilst the beginning of August appears to be about the time required by the history; so I contend that we need have little doubt on these grounds that the eclipse of August 3rd, B.C. 431, is the one alluded to.

Perhaps, for the full appreciation of other points, you will allow me to quote the passage of Thucydides in which the eclipse is mentioned. We need not stop to smile at the shrewdness which that historian evidently thought that he showed in noticing that an eclipse of the Sun always occurred about the time of New Moon. He says (book ii. c. 28):—

Τοῦ δ' αὐτοῦ θέρους νουμηνίᾳ κατὰ σελήνην, ὥσπερ καὶ μόνον δοκεῖ εἶναι γίγνεσθαι δυνατόν, ὁ ἥλιος ἐξέλιπε μετὰ μεσημβρίαν καὶ πάλιν ἀνεπληρώθη, γενόμενος μηνοειδὴς καὶ ἀστέρων τινῶν ἐκφανέντων.

The reason I have quoted this passage is that Mr. Johnson argues that the circumstances agree better with the eclipse of March 30, B.C. 433, than with that of August 3, B.C. 431, both because the former eclipse was larger in amount of obscuration, and because it occurred earlier in the day (soon after noon, whilst the latter was towards evening). But surely this last argument (which appears of most weight, as both eclipses were only partial), might much more forcibly be used the other way. For a partial eclipse would be much more likely to be noticed as thus described when the Sun was rather low in the heavens than when near the meridian; and, we may add, the visibility of a few stars (perhaps the expression is exaggerated, and only the planet Venus was really seen) would also be more easily explained during an eclipse, which, although partial, occurred whilst the Sun was at a small altitude in the sky. I for one see, therefore, no reason in this account for seeking to alter the ordinary historic date of the commencement of the Peloponnesian war, viz. B.C. 431.

Yours faithfully,

Blackheath, 1884, May 22. W. T. LYNN.

The Nature and Depth of the Dark Markings on Venus.

SIR,—

But little appears to be known concerning the nature of the dark spots visible on Venus, as to whether they may be depressions or elevations, or merely surface colourings, no doubt chiefly from the difficulty usually experienced in seeing them distinctly. It has been surmised, indeed, that a remarkable flattening of the circular limb was caused by one of the large dark spots seen in profile, and that consequently the spot would be of enormous depth, but apparently without much certainty.

During the past two months I have been paying considerable attention to this fascinating planet, which is rendered the more interesting from the difficulty with which the details of the surface can be seen; and although the results are chiefly negative as regards the depth of these large dark patches, still some few things were observed in connection with them, from which it appears that these objects actually are depressions, and from which it will be possible even to give an estimate, though of course one of extreme roughness, of their probable depth.

In ordinary cases when the dark markings are visible on the terminator, nothing peculiar is to be seen about them. The spot indeed appears darker, but it has no visible effect on the regularity or evenness of the curve of the terminator; and from this it would appear that these features are not generally of very extreme depth, such as would enable them to cause a noticeable flattening of the limb. But in a few cases when a large, very dark and plain spot was in this situation, it was seen to cause a slight, though distinctly perceptible, hollowing of the terminator. This was observed, for instance, on April 12th, at 7^h 10^m, when the west part of one of the darkest and plainest markings lately visible was just emerging into the enlightened part of the planet, and caused a plain, pretty deep, hollowing of the terminator.

The most conclusive evidence, however, of depth is afforded by a curious narrow dark band which has been visible since April 24th. It commences at the terminator just north of the south horn, and at the present time runs nearly parallel to the limb almost as far north as the middle of the west limb. It is usually an easy object to see, and is always visible in the present situation of the planet, which it completely encircles. This band generally causes a distinct small indentation of the terminator just north of the southern cusp; but occasionally at this point, when the band is unusually dark and marked, so as to be a conspicuous and striking object, there occurs not merely a slight indentation, but a deep narrow hollow, resembling the mouth of a river, a resemblance which is intensified by the dark narrow band continuing it onwards. How deep this indentation actually is at such times it is difficult to say, because the streak itself there appears almost black, but it certainly seems to run nearly $1''$ of arc into the bright part of the planet.

The dark spots are clearly of insufficient depth, however, to cause any visible flattening of the circular limb. I have frequently examined the outline of this planet carefully under most favourable circumstances as regards definition, and at the times when the darkest and plainest spots should be on the limb, but without being able to see the slightest certain indication of any such flattening, although when the air is unsteady fictitious flattenings appear at different points all round the limb.

It appears probable therefore, from my observations, that the depth of the deepest of the large dark spots does not exceed four or five miles, and that the ordinary spots probably do not reach to half that amount; whilst the depth of the deepest part of the dark band visible towards the south may amount to over 10 miles, perhaps considerably more, as this feature has not lately been in a position very favourable for ascertaining its depth. With regard to this last mentioned object, it is well known that Schröter sometimes observed the south horn to be rounded, with a detached bright point, from which he inferred the presence of a mountain as much as 27 miles in height. Now the position of this dark band or streak is such, that in a somewhat different situation of the planet it would, if of sufficient depth, cause some such appearance. It therefore appears probable that this phenomenon is caused, not by the presence of a mountain, or range of mountains, of this enormous elevation, but by a comparatively narrow valley or depression of perhaps nearly equal depth. It is possible, too, that this deep valley may, in a different position of the planet, cause a flattening of the limb, like that which has been observed near the south horn.

As to the real nature of these dark spots, there is of course little certainty. Since, however, we know, from various circumstances, that Venus has a denser atmosphere than ours, and that the spectroscope shows absorption lines, due probably to the presence of aqueous vapour in the atmosphere of this planet, it may be inferred that in the lower depths of these vast depressions water is present in considerable quantities, so as to form, in fact, oceans and seas. A. Stanley Williams.

West Brighton, 1884, June 3.

Jupiter.

Sir,—

The last two observations of the red spot, which I succeeded in obtaining during the past opposition, were as follows:—

	On C.M.	Longitude.
June 10	$8^h\ 3^m$	$102^\circ\!\cdot5$
12	9 35	98 ·5

Comparing these with an observation made here on 1883, Aug. 23, $16^h\ 48^m$, it appears that the rotation of the spot has been $9^h\ 55^m\ 39^s\!\cdot1$, which is the same as during the preceding apparition.

The dark transit of the first satellite on May 19, referred to in the 'Observatory,' No. 86, p. 176, really referred to the *fourth* satellite. The error arose from a mistake in the identification of the satellites and their shadows, several of which were presented on the disk at the same time. Yours faithfully,

W. F. Denning.

Bristol, 1884, June 20.

NOTES.

Royal Observatory, Greenwich.—The annual visitation of the Royal Observatory, Greenwich, took place on Saturday, June 7, when Mr. W. H. M. Christie, the Astronomer Royal, presented his Report to the Board of Visitors. The Report, which bears reference to the year ending 1884 May 20, informs us, under the head Buildings, Grounds, and Movable Property, that the new dome for the Lassell telescope was completed by Messrs. T. Cooke and Sons at the end of last March, and is in every respect satisfactory. It is 30 feet in diameter, covered with papier mâché on an iron framework, and turns with great ease; the arrangement of the shutter also appears to leave nothing to be desired as regards ease of manipulation. After the completion of the dome, the carpenters' work on the flooring &c. of the building and the attachment of the observing stage (which is fixed to the dome) have necessarily occupied much time, and the building is hardly yet complete in all details.

A 6-inch equatoreal by T. Cooke and Sons has been transferred to the Observatory by the Transit of Venus Committee, with the sanction of the Treasury, to replace the Naylor equatoreal lost in the 'City of Brussels.'

There is no change of importance to notice in the Transit-Circle, which has been kept in good working order. A reversion-prism for use with the collimators as well as with the transit-circle is being made by Messrs. Troughton and Simms. The Sun, Moon, planets, and fundamental stars have been regularly observed throughout the year, together with other stars from a working catalogue of 2600 stars, comprising all stars down to the sixth magnitude inclusive which have not been observed since 1860. Considerable progress has been made in obtaining the requisite number of observations of each star, and there is a good prospect that by the end of next year, when it is proposed to form a new Nine-Year Catalogue, the whole of the stars will be cleared off. The annual catalogue of stars observed in 1883 contains about 1550 stars.

The number of observations made in the year ending May 20 was—Transits 5213, Zenith Distances 4696.

The investigation of personal equations has been completed for the year 1883.

The discordance between the nadir observation and the mean of the results from reflexion observations of stars north and south of the zenith, which has gradually increased since the year 1879, still continues and has not yet been traced to its source. The mean correction to the nadir observation indicated by the observations of 1883 was $-0''\cdot45$, with considerable fluctuations in the course of the year, the mean discordance for the months of June and July being insensible. The correction deduced from the first four months of the present year is $-0''\cdot34$. Determinations of flexure by means of the collimators have been made on 1883

October 31, November 2, and November 8, the resulting values (found by three different observers) being $-0''\cdot39$, $-0''\cdot75$, and $-0''\cdot20$. The mean of five determinations made in 1883 is $-0''\cdot49$, whilst the mean of nine accordant determinations in the period from 1879 to 1882 is $+0''\cdot13$. The value of the flexure found in 1883 has the same sign as in the period from 1867 to 1877. The correction for R—D, the error of assumed colatitude, and the position of the ecliptic have been investigated, and the deduction of geocentric and heliocentric errors for the planetary results is complete for 1883. The correction for discordance between reflexion and direct observations of stars, deduced from observations in 1883, which extend from Z.D. 72° north to Z.D. 71½° south, is $-0''\cdot02+0''\cdot69$ sin Z.D. The assumed formula $a+b$ sin z represents the observations fairly well throughout the whole range of zenith-distance. The value found for the colatitude from the observations of 1883 is 38° 31′ 21″·86, very slightly smaller than the assumed value; the correction to the tabular obliquity of the ecliptic is $+0''\cdot15$; and the discordance between the results from the summer and winter solstices is $-0''\cdot32$. The mean error of the Moon's tabular place is only $+0^s\cdot03$ in R.A. and $+0''\cdot42$ in longitude, as deduced from the meridian observations of 1883. In this year Prof. Newcomb's corrections to Hansen's tables have been applied in the Nautical Almanac, so that the comparison has reference to Hansen's theory without his empirical term of long period (intended to represent the direct action of Venus) and with an empirical alteration in the epoch of the inequality resulting from the indirect action of Venus. The mean error in R.A. of Hansen's tables, uncorrected, was $+0^s\cdot82$ for the year 1882.

The observations with the altazimuth have been restricted to the period from last quarter to first quarter in each lunation.

The whole number of places of the Moon observed with the transit-circle is 109, with the altazimuth 79.

The Moon's diameter has been measured with the transit-circle once in R.A. and 15 times in N.P.D., and with the altazimuth twice in Z.D.

The mean solar clock, made many years ago by Shepherd and Son, has on several occasions in the past year stopped through failure in the electric contact made by the pendulum. Mr. Shepherd has proposed an improved form of electric escapement (described *post*, p. 211), which has been fitted to a pendulum placed in the North Dome for experiment. Meanwhile, in order to avoid the inconvenience caused by the stopping of the mean solar clock, which sends the hourly time-signals to the Post Office for general distribution, one of the Transit of Venus clocks (Dent 2012) has been fitted by Messrs. E. Dent and Co. with the requisite contact springs, so that it can be used at any time in place of the electric mean solar clock, and a relay adapted for driving the sympathetic clocks by means of the clock Dent 2012 is being made.

With the Reflex Zenith Tube about 50 transits of γ Draconis over the 30 wires have been taken in order to determine the scale-value corresponding to different temperatures and different readings of the focal length. It is proposed to continue these observations during the present year, with special attention to the focal adjustment.

Equatoreals.—The Lassell equatoreal has required a number of small repairs and general cleaning, some parts of the mounting having been probably strained in process of removal, the bearings in particular having suffered from wear and subsequent disuse, so that it has been necessary to raise the instrument and re-grind these in several instances. The mirror has been cleaned, and appears to be in very good condition as regards polish. The definition on stars seems to be very good as far as it has been practicable to test it before the mounting of the telescope has been put into proper order. The delay in the completion of the dome has necessarily delayed the work on the instrument, which is now rapidly advancing to completion.

With the Spectroscope attached to the S.E. Equatoreal the solar prominences have been observed on 21 days, and four sun-spots have been examined on 5 days with reference to the broadening of lines in their spectra. Displacements of the hydrogen lines in the spectra of the prominences and chromosphere, and reversals of the metallic lines have been noticed much more frequently than in preceding years. For the determination of motions of stars in the line of sight, 412 measures have been made of the displacement of the F line in the spectra of 48 stars, 91 measures of the *b* lines in 19 stars, and 2 measures of the D lines in one star, besides measures of the displacements of the *b* and F lines in the spectra of the east and west limbs of Jupiter, and in the spectra of Venus and Mars, and comparisons with lines in the Moon or sky spectrum made in the course of every night's observations of star-motions, or on the following morning, as a check on the adjustment of the spectroscope. Some preliminary measures have also been made of the F line in the spectrum of the Orion nebula. The progressive change in the motion of Sirius, from recession to approach, alluded to in the last two Reports, is fully confirmed by numerous observations since last autumn, and a change of the same character is indicated in the case of Procyon. A discussion of the measures of all the stars observed here shows that the results of the four periods—1875 June to 1877 May, 1877 June to 1880 December, 1881 January to 1882 March 10, 1882 March 11 to 1884 March 31, in each of which the instrumental conditions were different—accord generally within the limits of the probable errors, and that there is no systematic change from recession to approach, so that the presumption against error arising from defective instrumental adjustment appears to be strong. The spectrum of Comet *b* 1883 (Pons-Brooks) was examined on two nights, and at the request of Dr. Konkoly a search was made on five nights for bright lines in the spectrum of η Ceti, but their supposed existence was not confirmed.

In the twelve months ending 1884 May 20 photographs of the Sun have been taken on 219 days, and of these 507 have been selected for preservation. There were four days on which the Sun's disk was observed to be free from spots. The mean spotted area of the Sun was slightly greater in 1883 than during the preceding year, although the faculæ have shown a small falling off. For the year 1883 Greenwich photographs are available on 215 days, and Indian photographs filling up the gaps in the series on 125 days, give a total of 340 days out of 365 on which photographs have been measured. In 1882 the total number of days was 343, viz., Greenwich series 201 days, supplemented by Indian photographs on 142 days.

By the application of a new secondary magnifier and longer camera, the Dallmeyer photoheliograph has been adapted (since April 4) to take eight-inch photographs of the Sun instead of four-inch. A new and improved micrometer adapted to the measurement of photographs of the Sun up to 12 inches in diameter has been made by Messrs. Troughton and Simms, and is now used exclusively in the measurement of the solar photographs.

In 1883 there were only five days of great magnetic disturbance, as against 15 in 1882; but there were also about 30 days of lesser disturbance for which it appears desirable to publish tracings of the photographic curves. As proposed in the last Report, tracings of the photographic curves of magnetic movements and earth-currents, reproduced by photo-lithography on a reduced scale, have been given in the volume of "Greenwich Observations," 1882, for 34 days of greater or less disturbance, in substitution for the tables of ordinates measured from the photographic trace. Besides these, a brief description of all magnetic movements (superposed on the ordinary diurnal inequality) has been given for other days with a view of affording facilities for comparing them with solar phenomena.

The meteorological instruments and the Thomson electrometer have been maintained in good order. In the gale of Jan. 23 the short connecting chain attached to the pressure plate of Osler's anemometer gave way, having perished in course of many years' exposure to the weather; a new chain was substituted on Feb. 26. The flexible brass chain connecting the external chain with the recording pencil continues to give very satisfactory results.

A new photographic thermometer apparatus, so arranged that the dry and wet bulb traces shall fall on the same part of the photographic cylinder as regards time-scale, was almost completed by Messrs. Negretti and Zambra. By means of a long air-bubble in the wet-bulb thermometer, with a column of mercury above, the degrees and decades of degrees are registered for this thermometer just below the trace of the dry-bulb without any interference of the two records; the scale of time for the thermometers will be the same as for all the other registers, both magnetical and meteorological. A slight change has been made in the positions of the rain-gauges in the Magnetic ground. In the year 1883 the mean temperature was 49°·3, being 0°·4 lower than the average; the highest was 85°·1 on August 21, and the lowest 20°·6 on March 24, The mean monthly temperature was above the average in January and February, and below in March and July. In the other months it differed little from the average. The mean daily motion of the air was 291 miles, being 12 miles greater than the average; the greatest daily motion was 842 miles on December 12, and the least 62 miles on December 26. The number of hours of bright sunshine recorded by Campbell's sunshine instrument was 1241, which is about 30 hours above the average of the six preceding years. The aggregate number of hours during which the Sun was above the horizon was 4454, so that the mean proportion of sunshine for the year was 0·280, constant sunshine being represented by 1. The rainfall was 21·9 inches, being about 3 inches below the average.

Tracings of the barometer registers for the days following the Krakatoa eruption have been sent to Mr. R. H. Scott and to M. Paul Schreiber. Two series of atmospheric disturbances recurring at intervals of about 36 hours are recorded from August 27 to September 1. No definite connexion between magnetic or electrical disturbances and the phenomena of the remarkable sunsets of the past winter was remarked.

In all Departments the reductions are in a very forward state.

The number of Chronometers being tested at the Observatory is 192, of which 155 belong to the Navy, 33 are for the annual trial, and 4 are on trial for purchase by the Austrian Government. The first six chronometers in the trial of 1883 were slightly above the average of recent years as inferred from the trial numbers.

No failure in the automatic drop of the Greenwich time-ball has occurred during the year to which this Report refers. The ball was not raised on two days on account of the violence of the wind, and on four other days during the repair of the machinery.

The arrangement, referred to in the last Report, for sending a current to Deal and receiving a return-signal through the Chronopher of the Post Office telegraphs, was brought into operation on February 29, and has worked well since. The change has necessitated some slight alteration in the arrangements at Greenwich in order to receive the Westminster signal through the same wire which is used for the Deal current and its return signal. There have been 16 cases of failure in the dropping of the Deal time-ball owing to interruption of the telegraphic connexions. On 19 days the current was weak and required the assistance of the attendant to release the trigger, and on 9 days the violence of the wind made it imprudent to raise the ball. The errors of the Westminster Clock have been under 1^s on 53 per cent. of the days of observation, between 1^s and 2^s on 30 per cent., between 2^s and

3^s on 13 per cent., between 3^s and 4^s on 3 per cent., and between 4^s and 5^s on 1 per cent.

The only change in the personal establishment is that the Magnetical and Meteorological Assistant now ranks as a Second Class Assistant, a change which has removed an anomaly prejudicial to the interests of the Observatory.

The Astronomer Royal concludes with the following remarks :—

"In the past year the work of the Observatory has gone on steadily on the same lines as in former years, with such small extensions in certain directions as could be made without infringing the long-established principle that all observations are to be reduced and published without delay. A larger number than usual of meridian observations has been made, whilst the reductions are maintained in a very forward state. In the Spectroscopic and Photographic branch there has been considerable pressure during the long-continued maximum of Sun-spots, the work of measuring the photographs having been somewhat further increased by the adoption of large-scale photographs of the Sun. The preparation of the particulars of magnetic movements and of the plates for disturbed days has pressed somewhat heavily on the Magnetic and Meteorological department; but I trust that the facilities thus afforded for comparing magnetic and solar phenomena will be appreciated, and in ordinary years the labour involved will be very much less. I regret that this work has compelled me to postpone for a short time, amongst other things, the completion of the Meteorological Reductions to which I referred in the last Report."

Oxford University Observatory.—The Annual Report of the Savilian Professor was presented to the Board of Visitors of the University Observatory on June 5. With reference to his Lectures, Prof. Pritchard stated that the attendance in point of numbers had been greater than during any corresponding years, no fewer than 7 students and 2 ladies having attended the course on the Planetary and Lunar Theories throughout. In addition to Undergraduates, several students who had completed their course had attended the Observatory for exercise and improvement in some of the more recondite branches of Astronomy. The instruments had been kept in good working order, and some not very expensive additions had been made when necessary for special researches. After referring to the state of the observatory buildings, Prof. Pritchard said of the astronomical work :—"It is not without some personal gratification, which I anticipate will be shared by the Board, that no less than three Memoirs on important astronomical questions, and issuing from this Observatory, are printed in the last Volume (the 47th) of the Transactions of the Royal Astronomical Society—two of them from myself, and the third from the first assistant, Mr. Plummer, who has availed himself of Mr. Stone's recent Catalogue of Southern Stars to compute the probable motion of the Solar System in space. A fourth Memoir, the labour of three years, has, within the last few days, been communicated to the same Society, in which, I believe, I have from my own observations, combined with those of many other astronomers, demonstrated the existence of small displacements in the stars comprised among the Pleiades, arising, it may be concluded, from the mutual gravitation of a vast number of Suns, forming a system possibly outside our own. A very few years ago a series of careful measures of relative positions were made at this Observatory of some 250 stars in another cluster. These, I hope, will shortly be reduced and published for astronomers in a century yet to come, to make the same use of them which I have applied to observations of Bradley and others made a century ago. It occurs to me that philosophical investigations into cosmical astronomy such as these well befit the attention of an observatory established among institutions such as those in the University of Oxford. Lastly, upwards of a thousand measures of the relative brightness of stars have been made in the Observatory since the publication of the Memoir to which I have referred. There remain still about another thousand measures to be made furnishing observational work for both assistants, which I anticipate will be satisfactorily concluded before the next meeting of the Board. The measurement of all the stars from the Pole to the Equator and visible to the unaided eye will furnish a *Uranometria Nova Oxoniensis*, which I suggest may properly form a work suitable for publication by the University Press. I have hitherto purposely refrained from drawing much on the pecuniary resources of the University Press, and partly with that view I have preferred communicating from time to time the Astronomical work of the Observatory to the Royal Astronomical Society. I think that this 'Uranometria Nova' may advantageously to the University properly form an exception. At the last meeting of the Board it was suggested that as the Observatory was equipped with appropriate measuring apparatus of great excellence, my attention might properly be devoted to the determination of the Selenographical Longitude and Latitude of a large number of points on the Lunar surface, by means of the very valuable series of Lunar photographs in our possession. I have devoted some time to the consideration of the most judicious plan for so doing, and now that the heavy work of the Pleiades is disposed of, and the photometrical reductions arranged for, my attention will be at once concentrated on the Moon. I do not think that, at all events for the present, it will be necessary to apply to the Royal Society for pecuniary assistance, as was suggested at the last meeting of the Board."

The grant of £600 per annum for the maintenance of the Observatory expires this year; there is sufficient, by careful management, to provide for the requirements of the immediate future, but little or no margin is left for contingencies and none for books, "an indispensable element in all Observatory work;" fortunately, however, the Radcliffe Library is available through the courtesy of the Curator Sir H. Acland. The Report concludes with an acknowledgment of "the able and zealous cooperation of both the assistants, Mr. W. E. Plummer and Mr. C. A. Jenkins."

THE INTERNAL TEMPERATURE OF THE SUN*.—M. Hirn, in a paper recently communicated to the Paris Academy of Sciences by M. Faye, suggests that it is possible to obtain a rough idea of the temperature not merely of the solar photosphere, but also of the regions below the surface. Father Secchi has conjectured that the temperature of the photosphere may exceed 10,000,000°; but M. Hirn considers this estimate quite inadmissible, for the principal part of the solar light and heat is emitted from *solid* particles in a continual state of precipitation in a gas, and he thinks it certain that a temperature of 50,000° or 100,000° would convert the most refractory solid into the state of gas, so that this may be regarded as fixing roughly the higher limit of the surface temperature. But below the surface we have matter in a state of gas, much more strongly heated and much compressed. Portions of this interior gas are flung out from time to time in the eruptions watched by the spectroscope or seen during a total eclipse; and M. Hirn seeks to determine their original temperature by considering them as cases where a compressed gas is allowed to escape into a region of lower pressure. Where the pressure of the gas after its escape is exceedingly small compared with the pressure to which it was originally subjected, the formula for determining the speed of projection of the gas becomes

$$V=\sqrt{2g\,\epsilon\,c_p\,T};$$

where g is gravity, ϵ the value of the mechanical equivalent of heat, c_p the calorific capacity at constant pressure, and T the initial temperature of the gas. Taking the case of hydrogen gas, the value of V is calculated by two different methods, first by dividing the height reached by erupted matter by the number of seconds which it took to attain that height, and next by taking the greatest height attained in the eruption, and calculating the speed which an equal extent of fall represents on the Sun. These two methods both concur in fixing V as more than 200,000 metres a second, and T in consequence at about 2,000,000°. From the mode of computation, it is clear, M. Hirn remarks, that these results are minima, and that we must reckon by millions of degrees when the internal temperature of the Sun is in question.

MR. LEWIS M. RUTHERFURD of New York City has recently presented the Trustees of Columbia College with the valuable astronomical instruments of his private observatory. These are as follows:—A 13-inch equatoreal telescope, with mounting and clockwork complete; a photographic lens, with accessories for celestial photography; two micrometers for measuring double stars; four micrometers for measuring star-plates, a transit instrument and a

* Comptes Rendus, Vol. xcviii. No. 22.

sidereal clock. Mr. Rutherfurd also bears the expense of moving and remounting the instruments *.

CINCINNATI OBSERVATORY.—Prof. H. C. Wilson, Astronomer in charge of the Cincinnati Observatory,—Prof. Ormond Stone having become Professor of Astronomy at the University of Virginia and Director of the McCormack Observatory,—has recently issued the seventh volume of the publications of the Cincinnati Observatory. The three preceding volumes have been wholly devoted to double stars; the present one is occupied with observations of the comets of the years 1880, 1881, and 1882. The earlier part of the work contains determinations of position; the latter portion contains notes on the appearances of the various comets, the changes in the heads of comets 1881 *b*, 1882 *a*, and 1882 *c*, and the directions of the tails of the same three bodies. Dr. Bredichin's method is used in reducing and discussing these observations of the comets' tails. These physical observations are illustrated by ten full-page drawings.

DR. DRAPER'S PHOTOGRAPHS OF PLANETARY AND STELLAR SPECTRA.—A very interesting paper by Profs. C. A. Young and E. C. Pickering has recently appeared in the 'Proceedings of the American Academy of Arts and Sciences.' The earlier part of the paper, by Prof. Young, gives an account of Dr. Draper's experiments in stellar spectrum photography, and describes the various telescopes and spectroscopes he successively employed. The spectroscope with which all his successful plates were taken was constructed by Browning with two 60° prisms of dense (but white) flint glass, after the form devised by Dr. Huggins for stellar observations. The telescopes employed were a reflector constructed by Dr. Draper himself, with a mirror of silvered glass 28 inches in aperture, and two refractors, one of 12 inches, the other of 11 inches aperture, by Alvan Clark & Sons. The 78 plates in Mrs. Draper's possession were taken between 1879 Aug. 6, and 1882 Aug. 12, all of them upon Wratten and Wainwright's dry plates, to which Dr. Huggins had called Dr. Draper's attention during his visit to England in 1879. A complete list of these photographs is given in the paper, with the remarks upon them from Dr. Draper's note-book. Several of the plates were taken to the Harvard College Observatory in the spring of 1883 in order to be measured. The remainder of the paper is occupied by the measurements and their reduction. Photographs of the spectra of the Moon and of Jupiter were used as reference spectra, the wave-lengths of the lines being taken from Dr. Henry Draper's photograph of the diffraction-spectrum (Amer. Journ. Sci. cvi. p. 401). For wave-lengths too great to be contained in this map, Ångström's map was employed.

The paper concludes with a reprint of three papers on the subject of spectrum photography by Dr. Draper.

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* Sidereal Messenger, 1884, April.

HONGKONG AS AN OBSERVING STATION.—Dr. Doberck, the Government Astronomer at the new Hongkong Observatory, reports that the climate of the island renders it very suitable as an observing station to supplement the work of the English observatories. "The mean percentage of cloud is 64·2 for the months December, January, and February; 68·8 for the months March, April, and May; 61·5 for the months June, July, and August; and 51·8 for the months September, October, and November." It thus appears that the atmosphere is particularly clear during the autumn, while the spring is rather dull. Just the reverse of this obtains in England, where clear weather is common only in spring. The importance of this circumstance cannot be over-estimated from an astronomical point of view. It is a fact well known to practical astronomers that the part of the sky which is visible during the spring months in the evening in the United Kingdom has been specially investigated, while the autumn sky is still comparatively less known. It now appears that Hongkong is most favourably situated for observing during the autumn. So that not only can phenomena be watched there at an hour when they are invisible in England, owing to the difference in Longitude, but that even abstracting from Southern Constellations, the part of the Northern sky which it is most difficult to observe in England can be particularly well explored from that colony.

MR. SHEPHERD'S NEW ELECTRIC CLOCK.—An electric clock, designed by Mr. C. Shepherd to supersede the present Mean Solar Clock of the Royal Observatory, is now on trial at Greenwich. The present mean solar clock stopped occasionally during the past year through failure in the electric contact made by the pendulum; in the new clock the liability to stop, through failure of the contacts or batteries, is avoided by duplicating. The electrical power required is very small, and consequently the clock may be relied upon to go for a long time without attention.

The necessary motive power is obtained by causing the poles of a permanent magnet, shaped like the letter S, to be attracted and repelled by coils of wire, through which are passed reversed electrical currents measured by the pendulum. The alternate motion thus produced in the magnet is converted into a rotary motion by a light connecting rod and crank. The crank-shaft carries a fly, which is thus kept in continual motion, revolving in equilibrium, making one revolution to each double vibration of the controlling pendulum.

Two coils of wire are connected with entirely separate circuits, each circuit having two distinct batteries and contacts. The weight of the fly is adjusted (relatively to the magnetic power of the coils) so that whether only one battery or all four are in action, the motion will be kept up precisely the same; consequently the failure of some of the batteries, or their removal for replenishing &c., will not interfere with the going of the clocks. The rotary motion of the crank-shaft is used to give a step by step motion to the ordinary train of wheels required in a sympathetic clock, and also to raise the weight of a gravity escapement actuating the pendulum.

In the pendulum now on trial at the Royal Observatory, the gravity escapement is so constructed as to give the impulse in the middle of the vibration, and the electrical contacts are made and broken by the motion of the gravity escapement, so that the pendulum is perfectly detached.

HERÉNY OBSERVATORY.—Dr. Eugen von Gothard has issued the first volume of publications of the Astro-physical Observatory of Herény, Hungary. The principal instrument is a $10\frac{1}{4}$-in. Newtonian, which is furnished with complete apparatus for spectroscopic and photographic work. In addition to observations of Comets, the spectra and colours of nearly 300 stars were examined in 1881–2, and the stars classified according to Vogel's types. Observations of Mars and Jupiter accompanied by several good sketches, the solar eclipse of 1882 May 16, and the August meteors of the same year are also given. The observations for time determination are taken with a small portable transit; a set of meteorological instruments and two astronomical clocks form part of the equipment of the Observatory. A chemical laboratory and a workshop are located on the ground floor.

BELGIAN OBSERVATIONS OF TRANSIT OF VENUS.—The first part of Vol. V. of the Annals of the Brussels Observatory (new series) contains the account of the observations of the Transit of Venus observed with the heliometer designed by M. Houzeau and constructed by Mr. Grubb of Dublin. Two Cauchoix object-glasses of $0^{m}\cdot22$ aperture and $4^{m}\cdot34$ focal length and two of short focus were divided and a half of each mounted so that the images of the Sun and Venus were nearly equal, thus allowing an observation of the distance of centres to be taken by a single measure. At Santiago in Chili 606 observations were obtained in 308 min., at San Antonio in Texas clouds impeded the work. From a discussion of the measures taken, M. Houzeau obtains $8''\cdot911 \pm 0\cdot084$ for the mean solar parallax.

A TELEGRAM from Prof. Krueger states that a faint nebula, found with the Great Vienna Refractor on May 26 in R.A. $17^{h}\ 40^{m}\ 48^{s}$, Dec. $35^{\circ}\ 32'$ N., was missing on June 18; and suggests that it may be the Comet 1858 III., the return of which is expected during the present year. The brightness at the last observation in 1858 was, according to M. Schulhof, 2·0. On July 3 of the present year it is expected to be about 0·8; the comet must therefore be exceedingly faint.

MR. SADLER, in a letter to the Astr. Nach., says, in reference to the star H.G.C. 2094 ('Observatory,' No. 86, p. 173), "I have very little doubt that it is really an observation of D'Arrest, Nova 72; Herschel's Polar Distance being one degree too large."

Maxima and Minima of Variable Stars in 1884, July.

M, signifies maximum ; *m*, minimum.

July	1	S Libræ, M.
	1	R Aquilæ, *m*.
	3·3	X Sagittarii, M.
	3·5	T Monocerotis, M.
	3·6	U Sagittarii, *m*.
	3·8	W Sagittarii, *m*.
	5	U Monocerotis, *m*.
	6	R Sagittæ, *m*.
	6·6	U Sagittarii, M.
	6·9	W Sagittarii, M.
	7	R Lyræ, M.
	7·5	X Sagittarii, *m*.
	8	R Hydræ, M.
	9	R Vulpeculæ, *m*.
	9·3	W Virginis, *m*.
	10·4	U Sagittarii, *m*.
	10·4	X Sagittarii, M.
	11·4	W Sagittarii, *m*.
	13·0	β Lyræ, *m*.
	13·3	U Sagittarii, M.
	14	S Virginis, *m*.
	14	T Cancri, *m*.
	14·5	X Sagittarii, *m*.
	14·5	W Sagittarii, M.
	15	R Arietis, M.
	16	R Sculptoris, M.
July	16	R Coronæ, *m*.
	17·1	U Sagittarii, *m*.
	17·4	X Sagittarii, M.
	17·5	W Virginis, M.
	19·0	W Sagittarii, *m*.
	20	R Leporis, M.
	20·1	U Sagittarii, M.
	21·5	X Sagittarii, *m*.
	22·1	W Sagittarii, M.
	22·6	T Monocerotis, *m*.
	23·8	U Sagittarii, *m*.
	24·4	X Sagittarii, M.
	25·9	β Lyræ, *m*.
	26·5	W Virginis, *m*.
	26·6	W Sagittarii, *m*.
	26·8	U Sagittarii, M.
	27	T Capricorni, M.
	28	R Arietis, M.
	28·5	X Sagittarii, *m*.
	29	U Monocerotis, M.
	29·7	W Sagittarii, M.
	30·5	T Monocerotis, M.
	30·6	U Sagittarii, *m*.
	31	R Tauri, M.
	31·4	X Sagittarii, M.

Variables of Short Period.

δ Cephei.

	d.
July	2·45, *m*.
	4·05, M.
	7·75, *m*.
	9·35, M.
	13·15, *m*.
	14·75, M.
	18·55, *m*.
	20·15, M.
	23·85, *m*.
	25·45, M.
	29·25, *m*.
	30·85, M.

η Aquilæ.

	d.
July	5·7, *m*.
	8·1, M.
	12·9, *m*.
	15·3, M.
	20·1, *m*.
	22·4, M.
	27·2, *m*.
	29·6, M.

U Cephei, *m*.

		h	m
July	5	14	53
	10	14	33
	15	14	12
	20	13	52
	25	13	31
	30	13	11

Algol, *m*.

		h	m
July	3	11	28
	6	8	17
	20	16	21
	23	13	10
	26	9	59

λ Tauri, *m*.

		h	m
July	13	8	35
	17	7	27

δ Libræ, *m*.

		h	m
July	2	16	27
	7	8	10
	9	16	1
	14	7	44
	16	15	35
	23	15	9
	30	14	43

U Coronæ, *m*.

		h	m
July	12	15	36
	19	13	19
	26	11	1

Astronomical Memoranda, 1884, *July*.

Equation of Time:—Sun *after* Clock, July 1, 3^m 39^s; July 11, 5^m 16^s; July 21, 6^m 9^s; July 31, 6^m 7^s.

Sidereal Time at Mean Noon:—July 1, 6^h 39^m 46^s; July 11, 7^h 19^m 12^s; July 21, 7^h 58^m 37^s; July 31, 8^h 38^m 3^s.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
June 30....	15 50	8 17	357 19	+3 4	259 1
July 5....	15 54	8 14	359 36	3 36	192 51
10....	15 59	8 12	1 53	4 6	126 40
15....	16 4	8 9	4 8	4 35	60 30
20....	16 9	8 5	6 20	5 2	354 22
25....	16 16	7 58	8 28	5 27	288 12
30....	16 24	7 50	10 32	+5 50	222 5

The position-angle of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon.

Moon.	*sets.* h m		*rises.* h m		*rises.* h m
July 1..	12 14	July 11..	9 26	July 21..	16 18
2..	12 42	12..	9 52	22..	*sets.*
3..	13 13	13..	10 17	23..	8 6
4..	13 49	14..	10 43	24..	8 36
5..	14 31	15..	11 11	25..	9 3
6..	15 19	16..	11 43	26..	9 29
7..	16 13	17..	12 21	27..	9 53
8..	*rises*	18..	13 7	28..	10 18
9..	8 27	19..	14 3	29..	10 45
10..	8 58	20..	15 7	30..	11 15
				31..	11 49

Full Moon, July 7, 22^h 10^m; Last Quarter, July 15, 9^h 39^m; New Moon, July 22, 0^h 54^m; First Quarter, July 29, 10^h 1^m.

Mercury in superior conjunction with the Sun July 12^d 17^h.

Venus in inferior conjunction with the Sun July 11^d 14^h. Diameter:—July 1, 54″·2; July 31, 47″·8. Illuminated portion of disk 0·004 on July 15.

July 1,	R.A. 7^h 49^m·8,	Dec. 18° 34′ N.,	tr. 1^h 10^m,	sets 8^h 53^m
31,	6 49 ·3,	16 23 N.,	22 7	rises 14 41

Mars in Leo, and during the latter part of the month in Virgo, is in conjunction with the Moon July 26^d 10^h. Diameter:—July 1, 6″·2; July 31, 5″·7. Illuminated portion of disk 0·919 on July 15.

July 1,	R.A. 11^h 4^m·0,	Dec. 6° 54′ N.,	tr. 4^h 24^m,	sets 11^h 3^m
31,	12 8 ·7,	0 30 S.,	3 30	9 31

Jupiter between Leo and Cancer, in conjunction with the Moon July 23^d 0^h. Diameter:—July 1, 29″·7; July 31, 29″·1.

July 1, R.A. 8^h $40^m{\cdot}5$,	Dec. $18°$ $59'$ N.,	tr. 2^h 0^m,	sets 9^h 47^m
31, 9 6 ·6,	17 15 N.,	0 28	8 4

Saturn a morning star in Taurus, in conjunction with the Moon July 19^d 6^h.

July 1, R.A. 5^h $4^m{\cdot}7$,	Dec. $21°$ $27'$ N.,	tr. 22^h 22^m,	rises 14^h 19^m
31, 5 19 ·3,	21 43 N.,	20 38	12 33

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
July 12	37″·92	17″·08	25″·21	11″·36	15″·0

The south side of the rings is visible, the elevation of the Earth above their plane on July 12 being 26° 47′ S., and of the Sun 26° 37′ S.

Uranus.

July 1, R.A. 11^h $40^m{\cdot}7$,	Dec. $2°$ $54'$ N.,	tr. 5^h 0^m,	sets 11^h 19^m
31, 11 44 ·8,	2 26 N.,	3 6	9 26

Neptune.

July 1, R.A. 3^h $22^m{\cdot}3$,	Dec. $16°$ $45'$ N.,	tr. 20^h 39^m,	rises 13^h 5^m
31, 3 24 ·9,	16 54 N.,	18 44	11 11

Occultations.

	G. M. T. h m			G. M. T. h m	
July 3	8 57	ζ Libræ D. 139°.	July 15	12 53	*o* Piscium D. 99°.
11	11 18	θ Aquarii D. 98°.		13 51	*o* Piscium R. 301°.
11	12 29	θ Aquarii R. 302°.	27	8 16	B.A.C. 4294 D. 130°.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. Editor.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 88. AUGUST 1. 1884.

The April Meteors.

It must be confessed that in recent years the meteors of the April epoch have so moderated their intensity as to become inferior (as regards visible numbers) to many showers which are comparatively unknown, and which certainly have received far less attention. About twenty years ago the Lyrids constituted a stream of considerable strength and ranked in importance with the several other periodical showers which had been singled out as special in their character. But the activity of former years does not appear to have been sustained, so that our observers have tired of watching for a display which so rarely answered to expectation. We have no explanation of the origin of this apparent exhaustion of the stream further than that the richest portion of the meteor-cluster has probably removed from those particular regions of the orbit which have been traversed by the Earth in recent years. Like the Leonids of November, the abundant section may possibly be as far distant from the Earth as the planet Uranus, in which case we could hardly expect the system to afford any conspicuous sign of its existence, unless, indeed, the meteoric particles are condensed at several points along the orbit, or unless, similarly to the Perseids of August, they are very thickly strewn over a wide range of that orbit. But notwithstanding the admitted feebleness of this shower during some of its late apparitions, it none the less demands assiduous observation at every successive return, for, like the Leonids, it may become intensified to a considerable degree, though perhaps not attaining the splendour of former times. An intimation of this returning vigour is conveyed in the observations which have to be reported further on.

The Leonids have a period of little more than 33 years and present an orbital connection with the first comet of 1866 discovered by Tempel. As the comet nears perihelion the meteor-swarm increases in density; and if this be true of the April meteors we seem little likely to witness any great revival of the shower for many years, as the first comet of 1861, from which they are supposed to be derived, has a period of 415 years, according to

Oppolzer, and will not return to our parts of space until the eighth decade of the 23rd century. But it must be confessed that there is some degree of uncertainty attached to the association of comets and meteors, as the latter do not always fulfil the conditions required by theory. We should therefore be careful to pursue the practical part of the subject apart from such considerations and to accumulate facts, from every legitimate source, which, though apparently of little significance at the time, may ultimately prove of great worth, and perhaps necessitate the adoption of ideas more distinctly conformable with the undeniable issues of modern observation.

In regard to the Lyrids, there was a fine display in 1803, so that obviously the shower is to some extent independent of the comet of 1861, which at that epoch must have been situated at a vast distance outside the orbit of Neptune. The visible returns of this stream would seem to agree better with the period of 27 years formerly assigned by Herrick. A comparison of the ancient dates apparently bears out this conclusion, and the present year being indicated as a maximum, it remained for observation to determine how far the character of the shower confirmed the assumed period. An exceptional interest was on this account attached to the recent display, and additionally so as Mr. Corder had drawn attention to the fact that the shower in 1882 was much more active than usual, and my own observations had indicated its annually increasing richness. Under these circumstances it became very essential that the meteors should be suitably observed at their return in the present year, so that their numbers, directions, and appearances might be compared with such former apparitions as had been recorded.

At Bristol the night of April 19 was very clear in the early part. I began observing at 11^h and at $11^h\ 7^m$ observed a rather bright meteor, low in Serpens, and evidently a Lyrid. This was succeeded by several others at short intervals. At $11^h\ 28^m$ a brilliant 1st mag. meteor was noted almost stationary at $271° + 36°$, and its extremely foreshortened path, combined with such other meteors as were accurately observed, enabled the radiant-point to be determined with considerable exactness at $269° + 33°$. Between 11^h and 12^h I counted 17 meteors, all of them Lyrids, not a single shooting star being recorded from any other radiant during the hour's watch. Between $11^h\ 45^m$ and 12^h the sky was much involved in cloud, and only one meteor noted; at midnight very few stars were visible, and the observations could not be extended through the morning hours, as the clouds continued to gather and remained persistent until daylight. During the interval of perfectly clear sky from 11^h to $11^h\ 45^m$ the Lyrids were falling at the rate of 22 per hour for one observer, which represents a degree of activity beyond anything I had ever previously observed in connection with this stream, and proves the display of the present year to have been a very important one. Could the observations have been prolonged into the later hours of the night, when the radiant had assumed a more favourable altitude for the display of its meteors, it is probable that (unless the maximum had already passed) the shower would have developed into one of far greater intensity than that actually witnessed in the hour preceding midnight.

In point of magnitude, the 17 Lyrids observed were not of exceptional character, 2 being equal to 1st mag. stars, 3 equal 2nd mag., 4 equal 3rd mag., and the remainder very faint. The meteors were by no means swift; they were devoid of streaks or trains, and their visible flights generally short. In fact the individual features of these Lyrids are far from interesting; they lack the phosphorescent streaks of the Perseids, Orionids, and Leonids, and are equally destitute of the spark-trains of such meteors as the Taurids and Muscids. Their motions exhibit neither the surprising swiftness of the Gemellids of October or the very gradual flights, often most beautiful to observe, of such streams as the Andromedes of November. Indeed their short trajectories and appearances generally convey the impression that they are composed of a material capable of very brief sustenance when rendered incandescent by impact with our atmosphere.

The radiant-point, as I had determined it in 1878, 1879, and this year, is almost identical with the computed radiant of Comet I. 1861, the position of which, according to Prof. Herschel, is at $270^{\circ}\cdot5 + 32^{\circ}$. Schiaparelli gives the place at $270^{\circ}\cdot5 + 33^{\circ}\cdot5$. The mean position of the Lyrid shower in the three years mentioned was at $271^{\circ} + 32^{\circ}\cdot7$, which is extremely close to the cometary radiant. Mr. Greg's average of 8 various determinations (prior to 1876) was at $272^{\circ} + 35^{\circ}$. Since that year, however, several fresh observations have been obtained, and I have collected in the following table all the values I have seen:—

Epoch.	Radiant. °	°	Observer.
April 20	277	+38	Heis.
April 18–29	277	+34	Heis (12 meteors).
April 20	267	+35	Serpieri.
Mar. 19 (?) April 22	$277\frac{1}{2}$	$+34\frac{1}{2}$	Greg and Herschel.
1871 April 20	267	+35	Herschel.
1872 April 19–20	275	+32	Lucas (17 meteors).
1873–4 April 19–20	274	+37	Denning (32 meteors).
1874 April 19–20	268	+33	Konkoly (7 meteors).
1877 April 16–19	269	+37	Denning (7 meteors).
1877–8 April 19–20	275	+35	Corder (24 meteors).
1878 April 20–22	272	+32	Denning (13 meteors).
1879 April 19	275	+37	Corder (13 meteors).
1879 April 19–21	274	+34	Sawyer (10 meteors).
1879 April 20	272	+33	Denning (5 meteors).
1882 April 20	268	+37	Corder (26 meteors).
1884 April 19	269	+33	Denning (17 meteors).

Mean radiant-point at $272^{\circ}\cdot3 + 34^{\circ}\cdot8$ from 16 observations.

It is certain therefore that Mr. Greg's centre, derived by him from results obtained up to 1876, is well confirmed by the records which have accumulated since that year.

There are evidently several subradiants, or closely bordering showers, which are liable to be confused with the Lyrids and to give that display a somewhat ill-defined character of radiation. Prof. Herschel and Mr. Greg have called attention to one in Cerberus at about 270° +25°, and Mr. Sawyer, in 1879, noticed a companion shower from the point 272° +41½°. I have also seen a radiant of swift streak-leaving meteors from 287° +23°, and the same position is well confirmed by a number of paths recorded in foreign catalogues, chiefly in those of Dr. Weiss of Vienna. There are also other contemporary showers at 258° +37° and 294° +41°.

The true Lyrids are now giving decided promise of returning activity, and should be watched during the few ensuing years, for unless this periodical shower is sedulously observed at its visible returns we shall be unable to form any idea of its epochs of greatest intensity, which is a most important feature to ascertain.

W. F. Denning.

Bristol, 1884, July 1.

A uniform Ephemeris of the Clock-Stars *.

A noteworthy feature in the progress of our astronomical annuals is the rapid increase that has taken place within a few years in the number of the clock-stars. This increase seems to have been brought about chiefly by the influence of the Berlin list of 539 stars, which was published for the use of astronomers who were engaged in observing the zones of the northern heavens. The convenience of this list for the use of observing parties in the field seems to have led to the large increase that we find in the annual publications. For the year 1886 the 'Nautical Almanac' gives the mean positions of 198 stars, the 'Connaissance des Temps' of 316, the 'Berliner Jahrbuch' of 622, and the American Ephemeris of 383 stars.

That this great increase in the number of clock-stars has some advantages will not be denied, but, on the other hand, there seem to be some disadvantages that ought to be considered. In the first place the mean positions of the stars given in the different publications do not agree as well as might be expected when the great number of observations of these stars is taken into account and the number of years over which the observations are extended. These differences of position are indeed small, and their influence on the observed positions of other stars and planets cannot be great; but we have already examples in astronomy of the extension of

* Astr Nachr. No. 2602.

such small errors into large catalogues of stars, and of their development into periodic errors.

The constants for the reduction of the clock-stars from one epoch to another, and to apparent position, are now so well known, and the adopted values are so nearly the same, that the differences produced by the various reductions are frequently neglected; but even here it seems to me there is room for improvement. At the present time the values of the constants of reduction are known to a good degree of approximation, and it ought not to be difficult for astronomers to unite on a common system of values of these constants, and also on a uniform notation to be used in reductions. The great advancement in stellar astronomy made by Bessel in his 'Tabulæ Regiomontanæ' seems to have been partly disregarded and lost, and his methods for bringing the labours of many observers into harmony, and of making them all tend towards the advancement of the science, have sometimes been overlooked and forgotten.

Again, if a large list of clock-stars is at hand, the astronomer is apt to make his work purely differential. In many cases this may be his best course, but such work would be more easily reduced and compared if it could all be referred to a common system of clock-stars. The computation of systematic errors would not be avoided, but the work would be made easier and more certain.

In order to obtain an extensive and convenient ephemeris of the clock-stars, suitable for general use, it would be better to make such a work a special publication. In this way the adoption of the mean positions of the stars, and the selection of the constants of reduction and the system of notation, could be brought under the direction of one Office. The work should be so elaborate that the apparent position of a star could be interpolated with ease and certainty for any time, but even with such an extension the cost of the publication would not be great. I hope that the Astronomische Gesellschaft may be willing to undertake a work of this kind.

A. Hall.

Washington, 1884, May 30.

Neptune.

With reference to the communication from Prof. Pickering published in the last number of the 'Observatory,' it is to be regretted that he commenced his observations of Neptune in Dec. 1883, only about the time my observations showed that the rapid variations ceased.

His first observation was made Dec. 14^{d} $14^{h}\cdot3$ G.M.T., simultaneously with one of mine, and our estimates of the light agree within one-tenth of a magnitude; but here the comparison stops, unless indeed, the subsequent increase of light he noticed 1884, Jan. 21, corresponds with the increase I noticed Jan. 8. But any such increase or decrease would refer to irregular variation, and not to the rapid and regular variation previously seen, and assumed by me to be due to axial rotation.

MAXWELL HALL.

Jamaica, June 7.

The Problematical Satellite of Venus.

A VERY interesting contribution to the literature of this enigmatical body has recently been made by M. Houzeau (until lately director of the Royal Observatory of Brussels) in an article appearing in 'Ciel et Terre' for 1884, May 15. M. Houzeau is unwilling to believe that the cases in which a satellite was seen near Venus were all illusions, "for all these observations were made either by celebrated astronomers, such as Dominic Cassini, or at least by experienced observers," and it might be added that in the cases of the observations of Short and Roedkiær, the object was seen with more than one telescope, and with several different eyepieces. Short, indeed, actually measured the distance of the "satellite" from Venus with a micrometer, whilst Roedkiær's observations were confirmed on more than one occasion by the other astronomers of the Copenhagen Observatory. M. Houzeau cannot admit the body in question to be a satellite which only becomes visible under accidental circumstances, "first on account of the impossibility of properly representing the observed positions by an orbit described round Venus, and further because the mass of the planet deduced from the least defective attempts would be seven times the real amount."

Some years ago M. Houzeau had suggested that the problematical satellite might possibly be an intra-Mercurial planet. "Let a small planet, revolving within the orbit of Mercury, come on some occasion into so close an apparent approach to Venus as to be visible in the field of the telescope with it, and it would appear beside the larger disk of Venus as a body of smaller size, presenting almost the same phase as the great planet. This is precisely what has been observed."

"There was a method of deciding whether this explanation was admissible. An intra-Mercurial planet could not pass as far from the Sun as Venus does or even Mercury. It could then be seen near the former only at times, when, by its apparent motion, it was not very far from the Sun. If the observations were made sometimes at distances from the Sun in excess of the greatest distance of Mercury, it would be necessary to seek another hypothesis."

In order to test this theory, M. Houzeau therefore drew up the following table of seven of the best known and best attested instances in which the "satellite" of Venus is believed to have been seen:—

TABLE I.

No. of the Observation.	Date.	Venus morning or evening star.	Heliocentric longitude of Venus.	Elongation.	Geocentric latitude.	Distance from the Earth. Radius of Earth's orbit = 1.
1.	1645, Nov. 15	E	309°	31°	−2°·0	1·37
2.	1672, Jan. 25	M	162	46	+4 ·8	0·59
3.	1686, Aug. 28	M	59	38	−0 ·7	1·17
4.	1740, Oct. 23*	M	68	46	−0 ·5	0·60
5.	1761, May 7	E	207	34	+5 ·4	0·45
6.	1764, March 4	E	59	30	−0 ·7	1·38
7.	1764, March 28	E	98	35	+1 ·2	1·24

M. Houzeau points out that this table completely overthrows the suggestion he had made, since in every instance Venus was further from the Sun than an intra-Mercurial planet could possibly be.

The table, however, gives rise to another hypothesis. Expressing the dates in years and decimals of a year, in order better to ascertain the intervals between them, M. Houzeau obtains the following table:—

TABLE II.†

No. of the observation.	Date.	Interval.	Number of periods.	Length of period.
1.	1645·87	years.		years.
2.	1672·07	26·20	9	2·91
3.	1686·65	14·58	5	2·92
4.	1740·84	54·19	18	3·01
5.	1761·34	20·50	7	2·93
7.	1764·24	2·90	1	2·90
Total interval .		118·37	40	2·96

It will be observed that the successive intervals are multiples of very nearly the same period, which is almost exactly the length of the interval between the two last dates.

M. Houzeau asks, "Is this agreement, six times repeated, wholly the effect of chance? Undoubtedly it may be only accidental; but

* Old style. The line should read therefore:—

4. 1740, Nov. 3 M 86 46 +0°·7 0·64.

† The table as given by M. Houzeau is here altered for the true date of Short's observation.

the probability of the opposite view is so great that it will not be without interest to examine what a like periodicity would indicate, supposing it were established. There are two bodies, the one relatively large, and the other described as of much smaller dimensions, which appear side by side at nearly constant intervals. There can be no question that it cannot be a true satellite, since the two are separated in the intervals. It follows from the observed facts that the path they follow brings them together at fixed intervals of time. These paths are near one another throughout their entire length, for conjunctions have been observed at different parts of the orbit of Venus, on this and on the further side of the Sun, and to the east and west of that luminary. These conditions can only be satisfied by imagining two orbits sensibly concentric and of radii which differ but very little.

"To save time I will give the supposed star, which has to be mentioned several times, a name. Any name will do: I choose Neith, the name of the mysterious goddess of Saïs, whose veil no mortal raised. I will say then: Venus and Neith come again to apparent conjunction every 2·96 years, *i. e.* about 1080 days, in their concentric orbits. It is evident that Neith moves either quicker or slower than Venus, and that after having either gained or lost a revolution it is again at the same longitude as the latter."

The second hypothesis is shown to be the true one by reference to Table I., which shows that Neith and Venus have been seen together when Venus is at greatest elongation from the Sun. Neith must then be a planet a little exterior to Venus, and M. Houzeau shows must have a period of about 283 days. But if the period of Neith were 281 days, then five revolutions of Venus would exactly equal four revolutions of Neith, and the influence of the former upon the latter, which in any case would have been great, would occasion very considerable perturbations.

M. Houzeau suggests that two observations recently made at the Brussels Observatory may possibly be of this hypothetical planet Neith.

"If we add either 40 or 41 periods to the last date of 1764, we shall get very nearly the present time; but the interval being over a century, it is impossible to state exactly the time of conjunction. If we take, for instance, the first half of February 1884, say 1884·12, we should have for the period from the observation of 1764, 2·92 years, and from the first apparition of the satellite in 1645 2·94 years. I choose this date of February 1884, because at 6 P.M. on the 3rd of that month M. Stuyvaert, of the Brussels Observatory, saw on the disk of Venus, near the bright limb, an exceedingly bright point, which appeared like the satellites of Jupiter when crossing before the planet. This observation is rendered of more interest, as some days later M. Niesten noted a small star very near and a little to the S. of Venus, which seemed to consist of a nucleus and a very faint nebulosity, which he could not find again on following days. Was this a reappearance of the supposed tellite? Is there not ground for multiplying researches and for amining the disk of Venus and its surroundings daily?"

M. Houzeau discusses one objection to the theory as follows:— "The inclination of the orbit of Venus though not very great is still sensible. The observations of the supposed satellite have been made at various distances from the nodes of Venus, and consequently at points where the orbits must have been separated in latitude. However, in order to have the two bodies visible together in the field of a telescope there could not be more than about half a degree between them. On looking at the geocentric latitudes of Venus in Table I. for the different observations, it will be seen that they have very irregular values. To preserve the apparent proximity it is necessary to admit (which would be extremely improbable if the point in question concerned planets taken at hazard) that the two lines of the nodes as well as the two inclinations are very close—in other words, that the planes of the two orbits are very nearly coincident." But since the motions of Neith must be greatly influenced by the attraction of Venus, it is not improbable, M. Houzeau thinks, that its orbit may have been forced into practically the same plane as that of Venus.

This theory is so ingenious and would so neatly account for facts which at present seem inexplicable, that we might well wish it to be true. It would be something, too, to look forward to, that we might have a transit of Neith perhaps long before the next transit of Venus, a transit which would doubly excel in usefulness one of the larger planet, since it would give a higher factor for the solar parallax, and could be far more accurately observed. But a detail seems to have been overlooked in the theory, which we fear will prove fatal to it, viz. the fact that the mean distance of Neith from the Sun must, for the period of revolution assigned, be nearly ·84 of that of the Earth—in other words, that it will approach the Earth more nearly than Venus by about eleven millions of miles, revolving nearly midway the two, though a little nearer to Venus. It follows therefore that the condition that the orbits should be "of radii which differ but very little" is not satisfied, nor would Neith necessarily have the same heliocentric longitude as Venus when the two were in conjunction. Further, Neith could not revolve in the same plane as Venus; and as some of the observations (as 2, 4, and 5) were made when Venus was in the part of her orbit between the Earth and the Sun, whilst others were made when she was in the further part, no possible plane would satisfy all, though a small inclination of about 1° might serve for several.

It should be noticed that there are several observations extant, besides those here employed by M. Houzeau. Altogether there are about thirty that have fair claims for consideration. Amongst these are two series made at Copenhagen, the one in 1761 and the other in 1764, and detailed in 'Copernicus,' Vol. ii. p. 164, by Dr. H. C. Schjellerup. The entire series of the 1761 observations commences on Feb. 10 with the first of La Grange's observations, or, if these be rejected, with Montagne's on May 3, and extends to Aug. 13, the date of Roedkiær's last observation in that year.

Again, in 1764 we have Roedkiær's observations of March 3 and the following days, and Montbarron's of March 15, 28, and 29. It is impossible that the two planets moving at such different speeds should have appeared to continue in conjunction for so long a time, especially in 1764, when Neith, the more slowly moving body, must have been the further from the Earth.

It seems clear, therefore, that we cannot add Neith to our list of planets; yet the theory is noteworthy from its ingenuity and from its affording another example, in which the solar system appears prolific, of a purely accidental numerical coincidence, none the less accidental that the exclusion of Fontana's observation (so-called), which clearly arose wholly from the defects of his instrument, and the inclusion of Roedkiær's observation of 1768 Jan. 4, would have rendered it less perfect. It is curious, too, that this is the second such coincidence to which the observations of the "satellite" of Venus have given rise, since Lambert was able to combine all the observations in an orbit, which only failed by demanding a mass for Venus much larger than it really possesses. It might, however, be possible to revise Lambert's elements, since the mean distance of the hypothetical satellite which he obtained appears to rest mainly on Montagne's observations; and these were not measures but estimations, and are very greatly in excess of the distances as shown in the diagram in Baudouin's 'Mémoire.' To diminish Lambert's distance would, however, only remove one difficulty to create another, since it would make it more than ever incredible that the planet should have been scrutinized for 115 years, and watched during three transits, without revealing any trace of so close a companion if it had any real existence.

It seems impossible to suggest any new theory which shall be more successful than its predecessors have been. Uranus and the asteroids, all then undiscovered, might, it has been rather hastily conjectured, have one or other been in conjunction with Venus at these different dates. Uranus was indeed in the neighbourhood of Venus on 1764 March 4, and on that occasion alone, but not even then near enough to account for Roedkiær's observation on that day, whilst the minor planets are too small and too dull for the theory with regard to them to have any plausibility. There would seem to remain only the vague and unsatisfactory resource of ascribing as many of the observations as possible to false images, of regarding others as observations of stars, and in the case of the remainder, where the observations were too minute and precise to allow of these hypotheses, to adopt Webb's suggestion * of "atmospheric reflection or mirage." Perhaps, however, it is better to be content with things as they are, and to leave the "satellite" of Venus, at least for the present, as an unsolved "astronomical enigma."

* 'Nature,' vol. xiv. p. 195, where Mr. Webb gives a very interesting account of an observation of his own of a "satellite" of Venus.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Appulses of Venus to the Sun.

SIR,—

Early this morning an inferior conjunction of Venus with the Sun took place. A circumstance taking place so frequently might have been passed over without remark, only this is the conjunction that after fifteen periods produces the next transit of Venus on that distant morning in June 2004. On the present occasion, owing to the conjunction taking place below the horizon, the visibility of the planet, by means of its illuminated atmosphere, could not be seen in our own country. Distance from the limb of the Sun does not seem to make much difference in this matter, as the circle of the planet seems to have been seen by Mr. Prince at the conjunction in 1863, when Venus was 7° removed from the Sun. In December 1890, however, and in December 1898, the planet will be so near the Sun at conjunction that the point might be tested.

While speculating under what unknown circumstances the rare phenomenon in 2004 may be observed, it naturally occurs to us to look backward. None of our popular astronomical treatises mention the fact that the first time there was some expectation of seeing Venus on the Sun, at any rate in modern times, was not in 1631, but in 1611. In reality the planet was in "superior" not "inferior" conjunction at the time. While speaking of "superior" conjunctions, it may be remarked that an inspection of the Nautical Almanack for 1886 will show that in July of that year there is the reverse of a transit, viz. an occultation of Venus by the Sun. Dr. Dick, in his 'Practical Astronomer,' speaks of seeing Venus on October 2, 1843, "about half-past four P.M., only a few minutes after it had passed the point of conjunction," the instrument he used being a common non-equatoreal of 44½ inches focal length and power of 95. Venus at the time was only 58′ from the Sun's northern limb. If this was the case with so small an instrument, it may be a matter of astronomical curiosity to note how close to the Sun's limb the planet could be detected in 1886. A similar occultation appears to have taken place in 1765, midway between the two transits; but such must be a rare occurrence.

As to the expected passage of Venus in 1611, it may be as well to give an extract in full, from "Harriot's papers" in supplement to Bradley's miscellaneous works.

"Against the observation of Dec. 1, 1611, there is the mark (☌ ☉ ♀), and there can be no doubt that it refers to the conjunction predicted by Maginus, the notice of which is inserted with the same symbols against the same day in his Ephemerides. It appears to have excited considerable attention. Venus at that time was in the superior part of her orbit, and her motion was, therefore, direct. No transit could have taken place: but in spite of all which telescopes had laid open, there were some who still clung to the possibility of the Ptolemaic system, and they argued very justly that the doctrine of Venus being always below the Sun, would be established, if under such circumstances, it was seen upon its disk. Scheiner, therefore, discusses the event very particularly. In his second letter to Velserus, he points out that Venus was moving in a direction contrary to that in which the spots cross the disk: the one, therefore, could not be mistaken for the other. As Venus would advance with only the excess of her geocentric velocity above that of the Sun, the phenomenon, if it took place, would extend over a considerable time. According to Scheiner it was to commence at 11^h on the night of Sunday, December 11 (N.S.), and to last for at least forty hours. According to these numbers, the termination would not take place till 3^h in the afternoon of Tuesday the 13th. Monday the 12th was entirely cloudy, but, on the contrary, Tuesday was clear and fair from the earliest dawn till late in the afternoon. Scheiner, therefore, and others with him, took this opportunity of carefully observing the Sun from its first rising. Of course they saw nothing, and he, therefore, concludes that 'etsi cararemus omnibus aliis argumentis, hoc uno evinceretur Solem à benere ambiri.'"

More than a century after the prediction of Maginus, Charles Leadbeater, "Teacher of the Mathematicks," published a treatise on Eclipses, in 1731. In this he foretells the central ingress of Venus in 1761 to take place at 7^h 10^m A.M., central egress at 2^h 18^m P.M.; and in 1769 the central ingress of the planet at 1^h 59^m A.M., central egress 6^h 22^m A.M. How far from accuracy this was, all are aware who are acquainted with the observations of these transits.

Faithfully yours,
S. J. Johnson.

Melplash Vicarage,
Bridport, July 12.

The Veiled Solar Spots.

Sir,—

The interesting phenomena observed and so ably described by Father Perry before the Royal Astronomical Society on May 9th, published in the 'Observatory' (No. 86, p. 154), are evidently of the same nature and similar to those I myself observed for the first time in 1875, and a description of which I published in the 'Proceedings of the American Academy of Arts and Sciences'*, also in the 'American Journal of Science and Art"†, and, later, in the Manual accompanying "the Trouvelot Astronomical Drawings"‡. In plate i. of this series of drawings, which represents a group of sun-spots, I have attempted to illustrate the

* New series, vol. iii.
† Third series, vol. xi.
‡ Charles Scribner & Sons, New York, 1882.

type of large veiled spots named "subpermanent spots" by Father Perry.

As the veiled spots are not a very rare phenomenon, they were recognized by several observers soon after the publication of my paper; and, besides, that silent and trustful witness, the photographic plate, has also recorded them on several occasions, since I have often recognized veiled spots, which I had observed and drawn before with the telescope, distinctly reproduced upon these plates.

Since the publication of my paper, I have observed a great many veiled spots and, on several occasions, I have witnessed the transformation of large and small veiled spots into ordinary spots, and *vice versâ*. In a paper soon to be published, I intend to give a drawing of a veiled spot of important dimensions which I had observed as an ordinary sun-spot for several days before its transformation.

The veiled spots were undoubtedly much more numerous in 1875 than they have lately been, these spots being perhaps periodical, like the ordinary spots. If so, I think it probable that their maxima and their minima will be found to take place inversely to those of the ordinary spots; that is to say, that the maxima of the veiled spots will correspond to the minima of the ordinary spots, and *vice versâ*.

Father Perry, who had only an indirect and very limited knowledge of my paper, could not, of course, exactly understand what was meant by veiled spots; therefore the veiled spots he has described are not precisely those I have described. It seems evident that his veiled spots are those faint and pale greyish spots, always more or less visible, which are irregularly scattered about all over the solar surface. Although I have recognized and described these greyish forms marbling the solar surface (which also partake of the nature of the veiled spots), I have applied the term "veiled spots" more particularly to those among them which, in becoming momentarily darker, take the appearance of minute evanescent solar spots; and also to those still more characteristic which appear in the vicinity of the ordinary spots, and whose proportions and duration are generally greater. I particularly gave the name of veiled spots to these objects, because I wanted to indicate that they partake of the nature of the sun-spots, the granulations and the vapours of the photosphere covering them as with a veil. It is evidently to the larger veiled spots, seen in the vicinity of the regular sun-spots, that Father Perry has given the name of "subpermanent spots," in order to distinguish them from the faint greyish spots always and everywhere visible on the Sun, and which he designates the "veiled spots." Although these faint greyish objects partake, in a certain measure, of the nature of the veiled spots, yet I do not think they are regular veiled spots, but simply veiled spots in an embryonic state, which, according to circumstances, may or may not become regular veiled spots. In the future communication of which I have spoken above, I will express my views upon these faint greyish spots and their origin. It has been shown that the veiled spots are found in all latitudes, but, according to their position, they differ in size and sometimes also in duration. Those found in the high solar latitudes outside of the spot-zone are always minute and evanescent; while some of those appearing within the spot-zone are larger and sometimes more permanent, although a large veiled spot may be evanescent like the smaller type. But while the minute evanescent veiled spots are found as well within as without the spot-zone, so far I have never seen a large veiled spot outside the spot-zone. Another distinction exists: while, in general, the veiled spots seem to result from the development of the faint greyish forms described, sometimes they have a different origin, and result from the filling up of an ordinary spot by vapours and granulations of the photosphere. But whatever may be the way in which they are formed, as far as I can see they have the same appearance.

As far as I can judge from Father Perry's remarks, I do not see that he has observed that the veiled spots in process of formation are generally accompanied with small evanescent faculæ. Neither do I see that he has remarked that sometimes one, or several evanescent minute black spots (umbræ-like) are formed upon the greyish veiled spots. These minute umbra-like spots, with their greyish penumbra-like surroundings, and the small faculæ near them, have a strong family resemblance to the ordinary sun-spots, this resemblance being still more striking when these spots form into groups, as frequently happens.

It is a well-known fact that the ordinary sun-spots have a remarkable tendency to form into groups of various sizes; and I have shown elsewhere * that "whatever may be the number of spots thus assembled, the group is nearly always composed of two principal spots, to which the others are only accessories." The tendency of the spots forming a group to assemble into pairs seems to be general, and is even observed in the minute evanescent groups of veiled spots which are sometimes seen in the polar regions. I have also shown in the same publication that "whatever may be the position of the axis of the two principal spots when a group is forming, this axis has a decided tendency to place itself parallel to the solar equator, and, if it is disturbed from this position, in general, it soon returns to it when the perturbation has ceased." The minute and evanescent groups of veiled spots which I have observed in the high solar latitudes had evidently the same tendency to place the axis of the two principal spots parallel with the solar equator.

Although the single spots and the groups of veiled spots seen in high solar latitudes are evanescent and very small, the fact that they possess the principal characteristics distinguishing the ordinary spots is sufficient in itself, it seems to me, to show that these

* The Trouvelot Astronomical Drawings: Manual.

objects are real solar spots, differing from the ordinary sun-spots perhaps only in their smaller size and their shorter duration. I wish particularly to insist upon this point, because it bears directly upon the theory of sun-spots and on the constitution of the photosphere. So far, it has been admitted that the solar spots are the result of forces acting only on a central zone extending about 35° on either side of the solar equator. The evanescent spots which have been described, show clearly that this is not exactly the case, and that these forces, although they are much reduced in energy, act in the same manner all over the solar surface, and create spots even at the very poles of the Sun. Yours truly,

Meudon, 1884, June 25. E. L. Trouvelot.

Montaigne of Limoges.

Sir,—

Something has been said again lately regarding a possible satellite of Venus. Now although I do not know that we are called upon to subscribe positively to the opinion of Mr. Proctor ('Encyclopædia Britannica,' 9th edition, vol. ii. p. 792) that "there can be now no doubt whatever that Venus is without a satellite," and we are rather, it seems to me, in the same position on the question as we were with regard to a satellite or satellites of Mars before the year 1877, yet I conceive there is little doubt that no satellite of Venus *has ever been actually seen.* Had one existed at all approaching in size to the object which J. D. Cassini and Montaigne thought they saw, we should certainly be in no doubt whatever about it now, or be still in the position on the matter well expressed by Bailly ('Histoire de l'Astronomie Moderne,' vol. ii. p. 409), "on ne possède point ce qu'on n'est pas maître de retrouver." It will be remembered that Montaigne's observations were made in the year 1761, and were stated to be of an object the diameter of which was equal to a quarter of that of Venus.

Although therefore I cannot doubt that the objects seen by Cassini, Short, Montaigne, and others were merely ghosts, yet I cannot help thinking that it would be interesting to know more about Montaigne himself, as the discoverer of three comets, than appears to be at present within our reach; and I write this in the hope that some of the French astronomers amongst your readers may be able to supply something on the subject. Hitherto all I can find is that which is given in Lalande's 'Bibliographie Astronomique,' p. 477, where we read of Montaigne, under date 1761, "Il s'appelait Jacques Leibax; il naquit le 6 Septembre 1716 à Narbonne. Il avait été Doctrinaire dans sa jeunesse. Il a véçu longtemps à Limoges."

It is well known that Montaigne was the first to discover in 1772 the comet which at its third observed appearance in 1846 was found to be moving in an elliptic orbit of short period, and acquired the name of Biela's comet, from its discoverer on that occasion. Montaigne's discovery was made on the 8th March, 1772; and he afterwards discovered a comet (the only one observed that year) in 1774, on the 11th of August; also the second comet of 1780, which was discovered independently by Olbers on the same day, viz. the 18th of October.

Surely some further particulars would be interesting about Montaigne (Bailly and Pingré spell the name Montagne), of whom I have not been even able to ascertain the date of his death.

Yours faithfully,

Blackheath, 1884, July 2. W. T. Lynn.

The Eclipse of Pericles.

Sir,—

In an interesting communication in the July number of the 'Observatory,' Mr. Lynn takes exception to certain doubts I have expressed about the solar eclipse of B.C. 431 being that of Pericles.

W. D. Snooke, in his little work on this subject (published by Highley and Son, 32 Fleet Street), 1852, refers to the position of the brightest stars at the time. He remarks, "Lyra was high in the east or near the zenith, Arcturus high in the south-west, and Spica nearer the horizon; farther west were Regulus, and the planet Venus a little eastward of the Sun." A writer so accurate as Thucydides would hardly have said that "some of the stars shone out" if only Venus was meant; and it is perfectly certain that an eclipse of seven-tenths of the Sun's disk could bring out no star except the planet Venus: nor would this be even the case if she were only "a little eastward of the Sun." It seems impossible, therefore, that the magnitude of the eclipse of B.C. 431 was sufficient to cause any of the stars to shine out.

Yours faithfully,

Melplash Vicarage, Bridport, July 8. S. J. Johnson.

The Variable Star S Cygni.

Sir,—

A recent examination of my observations of the variable star S Cygni ($\alpha=20^{h}\ 3^{m}\ 6^{s}$, $\delta=+57^{\circ}\ 39'\cdot3$, 1885·0) has brought to light some irregularities, to which, with your permission, I should like to call attention.

In the annexed table I give my observed times of maximum, and the differences between the observed times and the times calculated from the elements given by Prof. Schönfeld in his 'Zweiter Catalog von veränderlichen Sternen,' viz.:—Epoch 1869, Jan. 20·1; Period 322·8 days.

I have, unfortunately, no observations between the years 1872 and 1877; but in No. 2066 of the Astron. Nachr., I find two maxima observed by Prof. Schönfeld, which partially fill the gap.

Max.	1874 May 9	$C-O+1^{d}\cdot 9$	Mag. 8·8
	1875 Mar. 30	$-0\cdot 3$	11·3

Max. Obs.	C–O.	Mag.
	d	
1863, Sept. 28	+ 4·3	9·4
1864, Aug. 20	+ 0·1	9·6
1865, July 16	− 7·1	9·6
1866, May 31	− 3·3	9·3
1867, April 21 ±	− 5·5	9·2
1868, Mar. 1 ±	+ 2·3	9·2
1869, Jan. 20	+ 0·1	9·3
1869, Dec. 9	− 0·1	9·6
1871, Sept. 18	− 2·5	10·0
1872, July 31	+ 3·3	9·0
1877, Jan. 5	− 1·3	9·8
1877, Nov. 5	+17·1	9·0
1878, Oct. 10 ±	+ 0·9	9·8
1879, Aug. 19	+10·7	9·2
1880, June 25 ±	+22·5	10·4
1881, May 6	+30·3	9·3
1883, Feb. 18	+22·9	9·2

It will be noticed that, between the year 1863 and the early part of the year 1877, the star appears to have gone through its changes with considerable regularity, the apparent errors being hardly larger than might be due to errors of observation. At the end of the year 1877 a change in this respect seems to have occurred, and the apparent errors in the six following years are large. A discussion of the last seven maxima, in the table, gives a period of 319·1 days, with epoch 1879, Aug. 15·6; but the residuals, though less than when Prof. Schönfeld's elements are used, are still considerable. The next maximum falls due, according to Prof. Schönfeld's elements, on December 17. Although not so deep in tint as some variables, the star is decidedly ruddy. A 9-mag. star closely follows it, rather less than 1′ north.

I am, Sir, yours faithfully,
GEORGE KNOTT.

Knowles Lodge, Cuckfield,
1884, July 17.

Average Amount of Cloud in England.

SIR,—

In accordance with your request I have much pleasure in submitting the following statement of the mean cloudiness of the sky at Greenwich. The monthly percentages of cloud, as found from observations made at the Royal Observatory during the past 30 years, 1854–1883, are as follows:—

January	73	July	66
February	74	August	65
March	69	September	62
April	64	October	68
May	65	November	69
June	67	December	74

These results show that a decided diminution takes place in the amount of cloudiness towards autumn, agreeing with the experience of practical astronomers, and disposing somewhat summarily of Dr. Doberck's statement (*vide* 'Observatory,' No. 87, p. 211) that "in England clear weather is common only in spring." During the period three instances of very low monthly percentages of cloud have been recorded in September, viz. in 1865=32, in 1854=42, and in 1870=43; two instances have been recorded in April, viz. in 1870=40 and in 1865=42; one instance in August, viz. 1871 =39; and one in June, viz. 1868=44.

Yours faithfully,
W. C. Nash.

Greenwich, 1884, July 23.

NOTES.

Paris Observatory.—The Report for the year 1883, presented to the Council of the Paris Observatory by the Director, Rear Admiral Mouchez, on 1884, January 30, has recently been published. The Director, before proceeding with the usual review of the work of the year, strongly recommends to the consideration of the Council the advisability of providing a branch establishment to the present Observatory. The meridian instruments have been used, as in the preceding year, for the reobservation of stars in Lalande's catalogue, and for the observation of the moon, the planets, and stars observed for comparison with the equatoreals; the number of observers has enabled three transit instruments and the meridian circle to be used continuously, when the state of the sky permitted. With these instruments 23,829 observations were secured (936 of the Sun, Moon, and Planets). The advanced state of the reobservation of Lalande's stars has necessitated the loss of much time from 8^h to midnight; in order to avoid this in future, it is probable that only two of the instruments will be used. Between 1882, December, and 1883, July, the division errors of the new meridian-circle were thoroughly determined; the flexure in R.A. and N.P.D. was investigated with M. Loewy's apparatus, but unfortunately the removal of the object-glass and micrometer was afterwards found necessary. The "Gambey" telescope has been furnished with an apparatus for determination of the error of collimation at the zenith, and, by a comparison of the errors at the zenith and horizon, the lateral flexure. The observations with the "Cercle du jardin" seem to indicate a variation in the collimation with change of temperature; should this be so, it offers a new proof of change in the position of microscopes with temperature.

Several important changes have been made in the 14-inch equatoreal. The object-glass lent by MM. Henry having proved very fine, the old wooden tube has been replaced by a metal one; a Foucault regulator has been applied and other modifications made in the clock-work, and a small gas-engine has been attached to the winch for turning the dome. The only alteration in the "equatoreal coudé" was the substitution of a new R.A. circle, as the former circle was too fragile. The other equatoreals have only required ordinary repairs.

The erection of the great telescope of $0^{m}\cdot74$ has been delayed by the impossibility of erecting it in the present grounds. A suggestion made by M. Bigourdan has been adopted and the side has been pierced to allow a spectroscope to be used by the aid of a mirror inclined at 45° to the axis of the telescope, the only change necessary when the telescope is to be used without the spectroscope being the removal of the mirror. The observations with the equatoreals comprised 400 observations of double stars, 284 observations of minor planets, comets, and satellites, in addition to the chart work by MM. Henry. Charts 39, 48, and 67 have been engraved, and 4600 stars have been observed in continuation of Nos. 12 and 66. MM. Thollon and Trépied were for some time at the temporary observatory established on the Pic du Midi; an account of their observations was given in the 'Observatory,' No. 81, p. 23. These observers consider the Pic du Midi admirably suited for observations, and advise that a small observatory be founded for use during the four or five months of the fine season; Admiral Mouchez hopes that funds will soon be obtained for the purpose.

In the new subterranean chambers of the magnetic observatory instruments have been erected, but the *matériel* is not quite complete. In the Meteorological Department the usual observations have been made regularly. A pneumatic anemometer, designed by M. Bourdon, forms part of the equipment of the Department; it is a modification of the tube of Venturi.

The museum has been enriched by the discovery of an interesting collection of instruments of the last century in the Toulouse Observatory, which have been repaired and placed in the museum at Paris. The following is a list of these instruments, with the dates of construction:—Transit by Lennel 1774, a telescope by Dollond, quadrant by Bird, quadrant by Canivet 1757, two quadrants by Langlois 1730 and 1746, a telescope of $3^{m}\cdot50$ with an iron quadrant graduated on copper, two micrometers, and a series of telescope-tubes. Portraits of Copernicus and Méchain and a bas-relief representation of the Transit of Venus have been obtained during the year, and the Director hopes to secure for the Observatory collection all the instruments &c. relating to astronomy now in the possession of the various national museums. During the year have been published the volume for 1880, vol. xvii. of the Memoirs, containing the Theory of the movement of Vesta (M. G. Leveau), a lunar inequality of long period due to the action of Mars (M. Gogou), Historical researches on the standards of weights and measures of the Observatory and the apparatus used in their construction (M. C. Wolf), Orbit of Comet Pons, 1812 (MM. Schulhof and Bossert), D'Arrest's periodic comet and its return in 1883 (M. G. Leveau), the ecliptic charts 39, 48, 67. The volume for 1881 and vol. xviii. of the Memoirs are being printed.

In 1884 it is proposed to continue the work of the catalogue and publication of current observations, and endeavour to gain some of the time lost in publishing the back volumes.

LIGHT OF VESTA.—The 'American Journal of Science,' vol. xxvi., contained an important communication by Prof. Harrington in which a value for the magnitude of Vesta was obtained from comparisons with the stars DM. $+22°$ 2163 and 2164. As the observations were made with a wedge-photometer, and were accordingly differential, the magnitudes of Vesta were therefore dependent on those assumed for the stars, which were taken from the DM. Prof. Pickering has recently undertaken the observation of these stars, with the meridian photometer of Harvard College Observatory, in order to provide means of reducing Prof. Harrington's results to absolute measures, and published his results in the 'American Journal of Science' for 1884, July. The corrections to be applied to the DM. magnitudes were found to be $+\cdot28$ for DM. $+22°$ 2163 and $+\cdot18$ for DM. $+22°$ 2164. The mean result for the magnitude of Vesta is 6·64. The values formerly obtained at Harvard College were 6·49 from observations on 12 nights in 1880, and 6·45 from observations on 10 nights in 1881–2; "the differences between these values do not seem large, considering the fact that the two methods of observations were very dissimilar. In measuring large intervals of brightness with the wedge-photometer, systematic errors may perhaps result from irregularities in the tint of the glass and other causes; on the other hand, the small meridian photometer used in observations of Vesta was not designed for measuring objects fainter than the 6th magnitude, and even the brightest asteroids were seen in the instruments with some little difficulty." The value 6·51, in vol. xi. p. 294, of the Harvard College Annals, was obtained by an indirect process, and Prof. Pickering considers that its close agreement with the results of 1880 and 1881–2 is probably accidental.

NEW METEOROLOGICAL JOURNALS.—Two new meteorological journals have lately appeared, one continental, 'Meteorologische Zeitschrift. Herausgegeben von der deutschen meteorologischen

Gesellschaft,' the other American, 'The American Meteorological Journal.' The former is edited by Dr. Köppen, and is similar in appearance and character to the Austrian 'Zeitschrift,' to which, judging by the contents of the first number, it appears likely to be a worthy companion. There are articles, amongst others, on the photographic observation of clouds, the daily period of the direction of the wind, and on types of weather, with notices of papers and books &c.; and altogether the journal promises to advance meteorological inquiry. The American journal (edited by Prof. M. W. Harrington, director of Ann Arbor Observatory) is of a somewhat different class, and appeals to the general, as well as to the scientific, reader. Three numbers have been published, in which are to be found original articles on barometric waves of short period, on the winds and currents of the equatorial Atlantic, and on the rainfall of Nebraska, the Oakville tornadoes, the sling thermometer &c., with other information on meteorological matters likely to be interesting to the general reader. Much that is noteworthy in American meteorology will no doubt find permanent record in this journal.

THE LIVERPOOL ASTRONOMICAL SOCIETY.—The second No. of the Transactions of this Society, containing the reports of the Solar, Planetary, Lunar, and Variable Star sections has recently been published. The Solar section contains a paper entitled "Solar Observation for Amateurs," no observations having been yet reported in this section, which has been but recently organized. The planetary report contains observations of Saturn, Jupiter, and Mars, by several Fellows of the Society. Amongst these it may be noticed that the Rev. J. J. M. Perry has measured the positions of Encke's division in the outer ring of Saturn, and finds it about one third of the breadth of the ring from the outer edge. Several drawings were made of Saturn, 56 of Jupiter, and 21 of Mars, one of each planet being reproduced in the Transactions. The attention of the Lunar section has been devoted to the study of Fracastorius and its neighbourhood, and it is proposed to map the district E. of Agrippa and Godin, for which purpose an outline map is here supplied. J. E. Gore, F.R.A.S., contributes several observations of suspected variables. Concurrently with these reports, the Society has published a valuable catalogue of the magnitudes of 500 stars situated in Auriga, Gemini, and Leo Minor, determined by the Rev. T. E. Espin from photographs taken at the Observatory of the Society. The instrument used is an equatoreal stellar camera, with a compound lens of $4\frac{1}{2}$ in. aperture and 15·8 in. focal length, mounted firmly on a wooden stand, and furnished with a driving-clock and slow-motion screws in R.A. and Dec. The plan adopted is to allow the images of the stars to move on the plate so as to appear as lines instead of dots. The small stars are lost by this method, but stars down to 8·5 mag. are secured with an exposure of half an hour. From a discussion of the measured magnitudes, Mr. Espin finds—(1) that the eye and photographic magnitudes are almost identical for two thirds of all the stars; (2) that the magnitudes of the 'Durchmusterung' are remarkably correct. Attempts have also been made to photograph various clusters and nebulæ. M 51 appeared as two misty patches, the larger showing central condensation, and distinct connection with the smaller nebulosity; the time of exposure was 2 hours. M 35 and the Præsepe have also been photographed. This catalogue reflects great credit on Mr. Espin; and altogether the Liverpool Astronomical Society is to be congratulated on the work it is accomplishing.

Dark Transits of Jupiter's Satellites.—Mr. J. H. Eadie, of Bayonne, New Jersey, writing to the 'Sidereal Messenger,' in reference to dark transits of Jupiter's satellites, says:—"On 1883, Dec. 3, I saw the fourth satellite and the shadow of the third, during a short observation with a $3\frac{1}{4}$-inch aperture, and power of 100, both in transit at the same time. The satellite was a little to the east and south of the shadow, and was on the bright band adjoining the bright belt south of the equator, and although of smaller size, was fully as black as the shadow of the third. I did not have time to watch this remarkable phenomenon for more than 15 or 20 minutes; but as others may have seen the beginning and ending of this transit, it would be interesting to hear from them on this subject." Messrs. Gildersleve and Hooper thus describe a dark transit of the first satellite on March 31:—"It was seen as a dark body on the edge of the S. belt, and at $3^h\ 30^m$ was more easily seen than its shadow, the air being very unsteady. The satellite was within one half an hour of egress, while the shadow was about one half an hour inward on the disc. The satellite continued dark until off the planet."

Comet *a* 1884*.—M. Cruls, Director of the Rio Janeiro Observatory, says with reference to this comet, that clouds prevented observations on the night on which he received the telegram from Kiel announcing its discovery at Melbourne; but on the following night, Jan. 17, it was found after a short search, and observations were made on several subsequent nights. These observations were not reduced, but M. Cruls sends the following notes respecting the appearance of the comet:—

Jan. 17. The nucleus appeared as a fine circular nebulosity with central condensation; tail seen, but very faint. The spectrum showed the three hydrocarbon bands, faint and diffused at the edges.

Jan. 18. Nucleus bright, tail $1\frac{1}{2}°$, spread out but limited on the right side (reversed image) by a more luminous rectilineal thread. Considering the motion of the comet, it is remarkable that the brighter and more clearly defined edge is the southern, in which

* Astron. Nachr. No. 2599.

direction the comet is moving, a peculiarity noted by M. Cruls in the great southern comet of 1882. On January 24 and 25 the general appearance was the same, as was also the spectrum; the intensity of light had become more feeble.

POLAR PROJECTION ON VENUS.—In the measurement of the photographs taken at Puebla during the Transit of Venus of 1882 Dec. 6, MM. Bouquet de la Grye and Arago have noticed a projection in the outline of Venus amounting to $\frac{1}{59}$ of the planet's radius, or about 65 miles. The photographs taken at Port-au-Prince also show the same projection; and M. Trouvelot points out that it occupies a position, so to speak, identical with that of the bright south-polar spot, which he has frequently observed (*cf.* 'Observatory,' No. 86, p. 176), and which often seemed to him to project beyond the limb. In 1878 January, after Venus had passed inferior conjunction, M. Trouvelot saw this spot several times and described it as composed of numerous bright peaks shown in profile on the sky. Drawings made by M. Trouvelot last January show the south-polar spot as very large and bright, whilst the spot near the north pole was scarcely visible. P. Lamey at Grignon has also observed the same south-polar spot, and seen it frequently project beyond the disk. He regards it as occasioned by volcanic mountains of great height, and alleges that the spot is surrounded by concentric circumvallations, recalling, but on a much larger scale, the great lunar craters. MM. Bouquet de la Grye and Arago suggest, with much greater probability, that the projection is due to accumulations of ice or great masses of cloud. If the latter, Venus must possess a much more extensive atmosphere than our own, although it is a smaller body; but both the appearance of the planet in the telescope and the evidence of the spectroscope concur to make that appear probable.

The measures of the French Transit of Venus photographs show a polar flattening for this planet of about $\frac{1}{303}$, not very different from that of the Earth.

A NEW FORM OF SPECTROPHOTOMETER.—Prof. E. C. Pickering delivered an address at the meeting of the Massachusetts Institute of Technology, on May 8, on the proper method of measuring colours, in which, after referring to the difficulty he had experienced with Vierordt's instrument, especially when the lights differed greatly in intensity, he described an improved form of spectrophotometer which he had designed and which he exhibited to the meeting. This consists of a spectroscope with two slits, in which the relative intensities of two spectra may be measured by polarized light. Special means have been adopted to render the edges of the images to be compared well-defined and of uniform brightness. The instrument can be used for the measurement, by absolute standards, of paints, dyes, &c. and for the comparison of lights from various sources. The chief difficulty in measuring the colours of stars is in the lack of light; in these measures Prof. Pickering's method, after spreading out the light of the star into a spectrum, is to employ a wedge, similar to that used by Prof. Pritchard, but instead of measuring, by means of a scale, the position on the wedge where the light is extinguished, the star is allowed to transit along the wedge, and the time of disappearance of each colour is noted. The exact colour is determined by a series of slits. All the spectra are brought into the same position by an auxiliary image brought into the field by means of a plate of plane glass cemented to the side of the principal prism. The results hitherto obtained are said to be such as to indicate that the instrument offers a satisfactory means of measuring the intensity of each part of a stellar spectrum.

Comet 1858 III.—Herr Spitaler, whilst sweeping on May 26 in search for this comet, with the great 27-inch telescope of the Vienna Observatory, observed three faint nebulous objects, all uncatalogued, in the following positions in R.A.—$17^h\ 40^m\ 50^s$, $17^h\ 42^m\ 0^s$, and $17^h\ 42^m\ 10^s$; the declination for all three being the same, $+\ 35^\circ\ 33'$. Clouds prevented further observations, and a long period of bad weather set in, so that it was not until June 17 that Herr Spitaler was able to examine these nebulæ again. Only the two latter could, however, be detected. It appears, therefore, not improbable that the first object may actually have been the comet, as it was not far from the place indicated in Schulhof's ephemeris. The missing object was faint, round, and with a perceptible central condensation. No further observation of the comet has been obtained.

Celestial Motions.—We are glad to welcome a second edition of Mr. Lynn's neat and handy little book. Having noticed the first edition so recently, it is only necessary to say now that, besides being carefully revised, the information has been brought up to the date of publication, and that a diagram has been inserted illustrating the motion of the November meteors in the solar system. In the chapter on comets a notice of Denning's is now included, so that a reference is made to every comet which has been known to return.

A New Minor Planet of the twelfth magnitude, No. 237, since named Cœlestina, was discovered by Herr Palisa at Vienna on June 27. Another, No. 238, was discovered on July 1 by Dr. Knorre at Berlin, whilst engaged in searching for No. 233.

A new Comet was discovered by Mr. E. E. Barnard, Nashville, Tennessee, on July 16, at $15^h\ 21^m$. R.A. $15^h\ 50^m\ 44^s$, N.P.D. $127^\circ\ 9'\ 52''$. Motion slow. It has since been observed at Washington and Algiers. M. Trépied observed it at the latter place on July 25, at $9^h\ 13^m\ 35^s$, Algiers M.T. R.A. $16^h\ 9^m\ 12^s$, N.P.D. $127^\circ\ 24'\ 14''$. No tail. Central condensation.

Prof. Lewis Swift discovered 5 new nebulæ during the night of May 17, three of which were near α Herculis. Prof. Swift hopes soon to publish a large list of nebulæ which he has discovered.

Maxima and Minima of Variable Stars in 1884, August.

M, signifies maximum; *m*, minimum.

Aug.	1	Z Virginis, M.
	1	R Crateris, M.
	1	S Aquilæ, *m*.
	3·7	W Virginis, M.
	6	R Leonis, *m*.
	6	S Hydræ, M.
	7	R Lyræ, *m*.
	7·8	β Lyræ, *m*.
	9	S Vulpeculæ, M.
	10	U Virginis, M.
	12·8	W Virginis, *m*.
	15	S Canis Min., *m*.
	16	R Capricorni, M.
	17·8	ζ Geminorum, *m*.
	18·6	T Monocerotis, *m*.
	20	U Monocerotis, *m*.
Aug.	20	T Herculis, *m*.
	20	R Coronæ, M.
	20·7	β Lyræ, *m*.
	21	R Draconis, M.
	21	R Comæ, M.
	21·0	W Virginis, M.
	22	R Lyræ, M.
	22·9	ζ Geminorum, M.
	25	L^2 Puppis, *m*.
	25	T Arietis, M.
	26·5	T Monocerotis, M.
	28·0	ζ Geminorum, *m*.
	29	V Coronæ, M.
	29	T Delphini, M.
	30·1	W Virginis, *m*.
	31	W Herculis, *m*.

Variables of Short Period.

δ Cephei.

	d.
Aug.	3·65, *m*.
	5·25, M.
	8·95, *m*.
	10·55, M.
	14·35, *m*.
	15·95, M.
	19·75, *m*.
	21·35, M.
	25·05, *m*.
	26·65, M.
	30·45, *m*.

Algol, *m*.

		h	m
Aug.	9	18	3
	12	14	52
	15	11	41
	18	8	30

η Aquilæ.

	d.
Aug.	3·4, *m*.
	5·8, M.
	10·6, *m*.
	13·0, M.
	17·8, *m*.
	20·2, M.
	25·0, *m*.
	27·3, M.

U Sagittarii.

	d.
Aug.	2·6, M.
	6·3, *m*.
	9·3, M.
	13·1, *m*.
	16·0, M.
	19·8, *m*.
	22·8, M.
	26·6, *m*.
	29·5, M.

W Sagittarii.

	d.
Aug.	3·1, *m*.
	6·3, M.
	10·7, *m*.
	13·9, M.
	18·3, *m*.
	21·5, M.
	25·9, *m*.
	29·1, M.

X Sagittarii.

	d.
Aug.	4·5, *m*.
	7·4, M.
	11·5, *m*.
	14·4, M.
	18·6, *m*.
	21·4, M.
	25·6, *m*.
	28·4, M.

δ Libræ, *m*.

		h	m
Aug.	6	14	17
	13	13	51
	20	13	25
	27	12	59

U Coronæ, *m*.

		h	m
Aug.	2	8	44
	12	17	18
	19	15	0
	26	12	42

U Cephei, *m*.

		h	m
Aug.	4	12	50
	9	12	30
	14	12	9
	19	11	49
	24	11	28
	29	11	8

Astronomical Memoranda, 1884, August.

Equation of Time:—Sun *after* Clock, Aug. 1, $6^m 3^s$; Aug. 11, $4^m 54^s$; Aug. 21, $2^m 50^s$; Aug. 31, $0^m 1^s$.

Sidereal Time at Mean Noon:—Aug. 1, $8^h 42^m 0^s$; Aug. 11, $9^h 21^m 25^s$; Aug. 21, $10^h 0^m 51^s$; Aug. 31, $10^h 40^m 16^s$.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Aug. 4	16 31	7 40	12 35	+6 10	155 58
9	16 40	7 34	14 23	6 28	89 51
14	16 48	7 22	16 10	6 43	23 45
19	16 57	7 12	17 49	6 56	317 41
24	17 4	7 3	19 21	7 5	251 36
29	17 10	6 52	20 45	7 11	185 33
Sept. 3	17 18	6 40	22 1	+7 15	119 31

The position-angles of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon.

Moon.	*sets.* h m		*rises.* h m		*sets.* h m
Aug. 1	12 28	Aug. 11	9 17	Aug. 21	7 4
2	13 13	12	9 48	22	7 30
3	14 5	13	10 23	23	7 55
4	15 3	14	11 5	24	8 20
5	16 5	15	11 55	25	8 47
6	*rises*	16	12 53	26	9 16
7	7 29	17	13 59	27	9 48
8	7 57	18	15 11	28	10 25
9	8 23	19	16 25	29	11 8
10	8 49	20	——	30	11 56
				31	12 51

Full Moon, Aug. 6, $11^h 7^m$; Last Quarter, Aug. 13, $15^h 8^m$; New Moon, Aug. 20, $9^h 54^m$; First Quarter, Aug. 28, $3^h 42^m$.

Mercury an evening star in Leo, at greatest elongation (27° 16′ E.) Aug. $23^d 5^h$.

Venus in Gemini, stationary Aug. $2^d 5^h$, at greatest brilliancy Aug. 17. Diameter:—Aug. 1, 47″·1; Aug. 31, 30″·1. Illuminated portion of disk 0·249 on Aug. 15.

Aug. 1, R.A. $6^h 49^m$·0, Dec. 16° 25′ N., tr. $22^h 3^m$, rises $14^h 34^m$
31, 7 41 ·9, 17 13 N., 21 1 13 27

Mars in Virgo, in conjunction with the Moon Aug. $24^d 4^h$. Diameter:—Aug. 1, 5″·7; Aug. 31, 5″·2. Illuminated portion of disk 0·938 on Aug. 15.

Aug. 1, R.A. $12^h 10^m$·9, Dec. 0° 46′ S., tr. $3^h 29^m$, sets $9^h 29^m$
31, 13 20 ·8, 8 31 S., 2 40 8 1

Jupiter in conjunction with the Sun Aug. $7^d 7^h$.

Saturn nearly stationary between Taurus and Gemini, in conjunction with the Moon Aug. $15^d\ 18^h$.

Aug. 1,	R.A. $5^h\ 19^m\cdot7$,	Dec. $21°\ 44'$ N.,	tr. $20^h\ 35^m$,	rises $12^h\ 27^m$
31,	5 30 ·3,	21 51 N.,	18 47	10 42

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Aug. 1	$38''\cdot75$	$17''\cdot44$	$25''\cdot77$	$11''\cdot60$	$15''\cdot6$
21	39 90	17 91	26 53	11 91	15 8

The south side of the rings is visible, the elevation of the Earth above their plane on Aug. 1 being 26° 45′ S., on Aug. 21, 26° 40′ S., and of the Sun 26° 39′ and 26° 42′.

Uranus.

Aug. 1,	R.A. $11^h\ 45^m\cdot0$,	Dec. $2°\ 25'$ N.,	tr. $3^h\ 2^m$,	sets $9^h\ 18^m$
31,	11 51 ·1,	1 44 N.,	1 11	7 24

Neptune is stationary Aug. $26^d\ 8^h$.

Aug. 1,	R.A. $3^h\ 25^m\cdot0$,	Dec. $16°\ 54'$ N.,	tr. $18^h\ 40^m$,	rises $11^h\ 7^m$
31,	3 25 ·6,	16 55 N.,	16 43	9 10

Phenomena.

G. M. T. h m		G. M. T. h m	
Aug. 3 7 32	B.A.C. 6292 Oc. D. 158°.	Aug. 14 13 34	B.A.C. 1351 Oc. R. 331°.
14 13 45	63 Tauri Oc. R. 294°.	15 15 21	115 Tauri Oc. R. 261°.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

Publications received:—*Washington Philosophical Society, Bulletin* Vol. vi.—C. Carpmael, *Canadian Observations of the Transit of Venus* 1882—M. Lamey, *Mémoire sur le Régime de Circulation de la Masse Fluide du Soleil* (Mémoires de l'Académie de Savoie); *Sur la Découverte du Système Géologique Eruptif de la Planète Mars* (Mémoires de la Société Eduenne)—G. Lorenzoni, *Sulle Determinazioni di Tempo Eseguite ad Arcetri nell' Autunno del* 1882 *colla osservazioni dei passaggi di stelle pel verticale della Polare* (R. Instituto Veneto di scienze)—J. Holetschek, *Ueber die Bahn eines Kometen, der während seiner günstigen Helligkeit nicht aus den Sonnenstrahlen heraustreten kann*—C. L. Prince, *Summary of a Meteorological Journal kept at Crowborough*—*Observations of the Great Comet of* 1882 *made at the U. S. Naval Observatory, Washington* (Government Printing Office, Washington)—*Note sur les influences thermométriques attribuées aux astéroides météoriques* (Bulletin Hebdomadaire de l'Association Scientifique de France, 1880, Aug. 1)—W. Huggins, *On some results of Photographing the Solar Corona without an Eclipse* (Spottiswoode & Co., London)—*On a Method of Photographing the Solar Corona without an*

Eclipse (Proc. Royal Soc., No. 223, 1882)—T. E. Espin, *A Catalogue of the Magnitudes of 500 Stars in Auriga, Gemini, and Leo Minor* (Liverpool, Meek Thomas & Co.)—*Transactions of the Liverpool Astronomical Society*, No. 2—E. C. Pickering, *Recent Observations of Variable Stars* (Proc. Amer. Acad. vol. xi. pt. 2)—Arthur Searle, *The Zodiacal Light—The Phases of the Moon* (Proc. Amer. Acad. vol. xi.)—L. Trouvelot, *On the Veiled Solar Spots* (Proc. Amer. Acad., 1875)—Elias Loomis, *Contributions to Meteorology*, Twentieth paper (Amer. Journal Science, vol. xxviii., July 1884)—Stonyhurst College Observatory, *Results of Meteorological and Magnetic Observations*, 1883 (Manresa Press, Roehampton).

ERRATUM IN No. 88.

Page 228, line 23, *for* benere *read* Venere.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 89. SEPTEMBER 1. 1884.

Meeting of the Liverpool Astronomical Society.

The third Annual Meeting was held on Saturday, August 16, at West Kirby, the Rev. T. E. Espin, B.A., presiding. The Secretary read a report on the state of the affairs of the Society. Since the last Annual Meeting, 114 Members and Associates, including the President and Officers of the Royal Astronomical Society, had been elected, and the Society had published 114 pages of printed matter, illustrated by 16 lithographs. Donations of 186 books and pamphlets had been received, and the publications of the Society had, in turn, been sent to nearly all the astronomical institutions in the world. As the Liverpool Society is composed largely of members residing at great distances, it had been judged only fair to keep the local expenses as low as possible; consequently, out of a total expenditure of £72, it had cost only 32*s*. for purely local expenses. The officers elected for the ensuing year were the Rev. T. E. Espin, B.A., F.R.A.S., President, and Messrs. Howard Grubb, F.R.S., R. C. Johnson, F.R.A.S., and Joseph Baxendell, F.R.A.S., Vice-Presidents. The Council included the Revs. Prebendary Webb, F.R.A.S., and Father Perry, F.R.S., &c. (of Stonyhurst). Mr. W. H. Davies, F.R.A.S., was re-elected Secretary, and Mr. J. Stead, Treasurer. 38 gentlemen were balloted for and duly elected Members, and E. W. Maunder, Esq., F.R.A.S., was elected an Associate of the Society.

The President then read a report of the work done at the Observatory. This, he said, consisted, during the past twelve months, chiefly of stellar photography. The large camera was set up in March, and from some of the plates the catalogue of 500 stars, recently published, had been taken. Attention had also been given to the photography of star-clusters and nebulæ; but perhaps the most important work had been the comparison of star-magnitudes. For this purpose the photographs had been taken at intervals of a month, and each photograph compared with those previously taken. If any star appeared to have varied in brightness, systematic observations of it were at once commenced, and several such stars were, at the present time, under observation.

In addition to the papers laid before the Society, communications had been forwarded to the 'Journal of Science,' the 'Observatory,' the 'British Journal of Photography,' and the 'English Mechanic.' Observations had been secured of some of the known variable stars, and also of other stars which were suspected to vary, some of the stars being also observed for colour. During the month of July the Observatory had been closed, and an expedition to the Rigi was undertaken, to carry out the research as to atmospheric absorption at low altitudes, suggested to the Society by Professor Pickering. Some 50 comparisons of stars with a greater zenithal distance than 80° were obtained, and the value of the results was much increased by the meteorological observations of Dr. Stierlin, which were made simultaneously with the stellar observations, and kindly placed at the disposal of the Society. These observations consisted of records of barometic, thermometric, and hydrometric readings at 9 P.M. and 7 A.M. The fact that these observations had been obtained was notified to Professor Pickering, and they had been placed at his disposal. No satisfactory determination of the atmospheric absorption had, he believed, yet been made at a greater zenithal distance than 80°. The absorption of the light of Capella from observations on four nights, at its lower culmination, was as follows:—

Date.	Z. D.	Absorption.	Barometric.	Thermometric.	Hydrometric.
July			millim.	Centigrade.	
13	87·5	2·17	630·8	18·0	50
14	87·4	2·70	630·6	15·0	50
22	87·5	2·17	628·5	10·0	80
23	87·5	2·17	628·0	12·0	60

These results show a mean of

Z. D.	Absorption (in magnitudes).
87°·5	2·30

Several observations of variable and suspected variable stars were also made on the Rigi. The applications for assistance in observational astronomy had been very considerable. There had also been a fair number of visitors.

M. Casimir M. Gaudibert, in a short paper on lunar observations, strongly advised Members to obtain the best maps before they commenced a systematic study. A very large number of sketches, more or less perfect, were often made at the telescope, and found afterwards to be, at best, a reproduction of objects already clearly defined in maps. This continued repetition took a great deal of time, which could often have been saved by a previous reference to the locality in a map. Besides, good opportunities were scarce, and, it seemed to him, that if it were possible to obtain separate portions of some of our best lunar maps, and compare them with the Moon, adding what was missing, and correcting positions of objects, &c., the work would progress more quickly and with greater certainty. He thought Schmidt's map the best, though still capable of improvement; but as he could certify from his own experience that few, if any, observers had looked at the Moon so closely as Schmidt, and as the details had been drawn with the help of the finest telescope in Europe, he would advise Members to pause before they decided him to be wroug.

The Secretary announced that, in future, the Meetings would be held on the second Monday in each month, instead of on the third as heretofore. Cards with the date of meetings would be printed and distributed for the convenience of Members. The Observatory and instruments, including the large stellar camera, were then inspected, and the meeting adjourned.

Stellar Photography.

A FEW words on the photographs taken with our Stellar Camera may perhaps not be unacceptable to your readers. The instrument was placed at the disposal of the Liverpool Astronomical Society by Howard Grubb, Esq. Mr. Grubb had been much interested by the preliminary attempts at photographing large fields of stars, and a correspondence between the Secretary (Mr. Davies), Mr. Grubb, and myself led to an offer on his part to place at our disposal a powerful and fully equipped instrument for a mere nominal charge.

After much thought and trouble Mr. Grubb forwarded the instrument, which was erected at once at West Kirby, and with which a series of trial photographs were taken before commencing a systematic zone of work. From these photographs a catalogue of the magnitudes of 500 stars has lately been published. In so new a line of research, unexpected difficulties have constantly arisen, and so far the work accomplished must be looked upon as purely preliminary. Probably the most remarkable point is the astonishing space penetrating power of the lens, which is a compound one of $4\frac{1}{2}$ inches aperture. After an hour's exposure stars uncatalogued by Argelander begin to appear, and in one or two of the excessively long exposures it is impossible at present to say how faint the smallest star is. Wratten and Wainwright's plates have entirely superseded those of all other makers, and for certainty and rapidity promise to keep the first place for stellar photography. Although no especially good photographs of double stars have been taken, yet one or two have been photographed. On the night of April 26, a plate was taken of part of the constellation of Lyra. The famous pair ϵ^1, ϵ^2 came out both of exactly the same magnitude; but in the case of the neighbouring star, ζ, a curious reversal of magnitude is observable, for instead of the magnitudes being as

x 2

they appear in the telescope, viz. A $4\frac{1}{2}$, B $5\frac{1}{2}$, we have A=$6\frac{1}{2}\pm$, B $4{\cdot}0\pm$. The larger star of this pair is yellow, the other blue. So intense is the actinic light of B that it comes out brighter than ϵ^1, ϵ^2 Lyræ. Another curious instance occurs in the case of the fine pair B.A.C. 6468, the magnitudes of which are given by Otto Struve, according to Webb, as 5·1, 7·1. The pair was photographed on the same night as the preceding pairs; but the two stars instead of being reversed are both reduced to the same brightness, and are both less than 7·0 magnitude by at least $\frac{1}{2}$ a magnitude, if not more. The fine wide pair δ^1, δ^2, on the contrary, undergo a marked reversal, for in the telescope the magnitudes are A $4\frac{1}{2}$, B $5\frac{1}{2}$; but on the photograph we have A=$6{\cdot}3\pm$, B $5{\cdot}0\pm$. In a photograph taken of α^1 and α^2 Ceti, the stars are reversed, the light of the large star being reduced, while that of the bright one is increased.

The photographs are indeed principally valuable for the reduction of magnitudes of the stars. For this purpose the stars are allowed to drive so that they come out on the plates as lines, which are more easily compared than dots. A full explanation of the method will be found in the preface to the catalogue. There can be no doubt that the photographic impression is nearly equal to the eye magnitude in the case of two thirds of the stars. The other third fall into one of two classes; the bluish stars increase in magnitude, while the reddish ones decrease.

The remark of Dr. Gould that most of the stars are liable to fluctuations in light, is certainly not demonstrated by the photographs; but of the 500 stars whose magnitudes have been reduced from the plates, only two cases of possible variation have been detected. In the zone work now just commenced the plates of the same region will be taken, at least, one month apart, in order to test this conclusion of Dr. Gould more minutely. It is hoped also that new variable stars may thus be brought to light. Attempts will be made to photograph some of the stars on whose variation doubt has been thrown. By photographing them and the comparison star on several nights all doubts as to their variation will be removed. The effect of scintillation on the star-images is not without interest. The action of the light on the plate is twofold. The first and greatest action is at right angles to the surface of the plate. This of course is a penetration of the film by the light, the depth of marking being in accordance with the strength of the actinic light of the star. But the plate is also affected perpendicularly to this line of action by atmospherical disturbance of the star's light, and the resulting indenture of the film thus becomes conical or funicular. The size of the images of the stars then is greatly dependent on the state of the atmosphere.

These disturbances are sometimes excessive. On one occasion, for instance, the image of α Coronæ was enlarged up to $\frac{1}{10}$ of an inch. Here is a comparison of the images of five of the stars in Corona on two different nights:—

	April 15. $\frac{1}{10}$ inch = 1·0.	April 30. $\frac{1}{10}$ inch = 1·0.	Photographic magnitude.
α......	1·0	0·6	2·0
γ......	·5	·4	3·7
β......	·5	·2	4·1
θ......	·4	·3	3·8
δ......	·2	·1	4·6

According to these two plates, if the determinations are taken from the size of the images, the magnitudes would be different on different nights. These results led to the abandonment of attempts at determining the star's magnitude by the size of the image. Instead of this the stars are allowed to drive. It was found that stars down to the seventh magnitude left a mark upon the plate in transit if the clock was stopped. These lines are sharp and clear and are unaffected, except in the case of the brightest stars, by atmospherical disturbances.

The plates are so sensitive that the Moon, however distant from the part of the heavens on which the camera is turned, fogs them; but, except just at full Moon, the fogging is not sufficient to prevent stellar photography with short exposures. The Moon has frequently been photographed instantaneously in the focus of the 5-inch refractor, with Wratten and Wainwright's plates. I am not aware that a photograph has ever been taken of the Moon with so small an aperture; these photographs are seven-tenths of an inch in diameter.

Such are some of the preliminary results obtained with the instruments at present in our possession. Incomplete as they are I yet hope they may not be without interest to your readers, and may lead others to take up the work of stellar photography. A new field is open for research, and the instruments necessary are within the means of many of our amateurs. In conclusion, I may state that the zone work is actively progressing, and a zone with a Declination +40° has been chosen.

T. E. Espin, B.A., F.R.A.S.,
Special Observer to the L.A.S.

Statistics of Stellar Distribution derived from Star-Gauges and from the Celestial Charts of Peters, Watson, Chacornac, and Palisa.

In Volume II. of the 'Publications of the Washburn Observatory' now printing, I have collected all the data available for a discussion of the distribution of the fixed stars, so far as this can be determined by the method of star-gauging.

The data have been collected for a comparison with the results of a series of star-gauges which are in progress here, with the 15-inch equatoreal. The tables are printed now, in order that they may be available to others besides myself. They include :—

I. The 683 previously published gauges of Sir W. Herschel; with the places brought up from 1690 to 1860.

II. The 405 unpublished gauges of Sir W. Herschel, which have been extracted from his observing books, and generously placed at my disposition by Lieut.-Col. John Herschel. These have also been reduced to 1860.

III. 500 counts of stars from the published star-charts of Dr. Peters.

IV. 983 counts of stars from the unpublished charts of Dr. Peters; from the Paris charts as revised and completed by him; and from the unpublished ecliptic charts of Professor Watson.

V. 856 counts of stars from the unpublished and published charts of Dr. J. Palisa.

Besides these, the data from Sir J. Herschel's (605) southern gauges and Celoria's 'Durchmusterung' of the stars between 0° and +6°, have been reduced to 1860 and compared with the tables. These occupy about 140 octavo pages, and have, I conceive, a permanent value.

I have thought that the two tables following, which summarize them, might be of general interest. I desire to express my obligations to Dr. Peters and to Herr Palisa for the data of their admirable star-maps.

In Table A the first column gives the authority; the second, the number of gauges, or counts; the third, the number of separate pointings of the telescope; the fourth, the total number of stars counted (for Palisa and Peters, the *actual* number of stars counted in each 4^m of R.A. by 1° in Dec. has been multiplied by sec. δ before making the summation); the fifth, the average number of stars per count, or gauge; the sixth, the average number of stars per square degree of a great circle; the seventh, the limits of magnitude (approximate) of the stars counted; the eighth, the number of stars down to each certain magnitude, which would be scattered over the whole surface of the sky, *provided* the distribution over the whole sky was the same as that in the special area counted; the ninth, the *limits* of declination of the special areas considered (the *limits* of R.A. being 0^h and 24^h in each case, though the whole 24^h in R.A. is only investigated in the first 8 horizontal lines); and the tenth, the actual number of square degrees which have been counted, and upon which the numbers in the eighth column depend.

The table furnishes several simple proofs that the stars cannot be regarded as equably distributed in space. The gauges of the elder Herschel numbers 13 and 14, for example, may be taken as two fairly comparable determinations of the same quantity. Any satisfactory formula should cover these two cases, as indeed it should cover the gauges of Sir John Herschel. The first two determinations (series 1 and 2) differ from each other by over 30 *per cent.* of the smaller, and hence no expression of the form $\Sigma_m = a\, b^m$ will serve to represent the numbers of col. 8 throughout, though such a one may serve as far as the 11th magnitude. The limiting magnitude in col. 7 will be fixed by the photometric determinations of the Harvard College Observatory, except perhaps those corresponding to the Herschel gauges.

TABLE A.—*Statistics of Star-Gauges.*

No.	Authority.	Number of counts or gauges.	Number of fields.	Total number of stars counted.	Average number of stars per count or gauge.	Average number of stars per 1° square.	Limits of magnitude.	Proportional number of stars in the whole sky.	Limits of declination of the counts	Number of square degrees actually counted.
1.	Argelander: Uran. Nova	....	....	2,352	....	0·11	1— 6?	4,704	+90°; 0°	20,626·5
2.	Behrmann: Atlas	....	....	2,328	....	0·17	1— 6?	6,964	−20; −90	13,790
3.	Houzeau: Uran. Gen.	....	....	5,719	....	0·14	1— 6?	5,719	+90; −90	41,253
4.	Heis: Atlas Coel.	....	....	3,903	....	0·19	1— 6·5?	7,806	+90; 0	20,626·5
5.	Gould: Uran. Arg.	....	....	6,694	....	0·32	1— 7·0?	13,388	0; −90	20,626·5
6.	Argelander: DM.	....	....	25,321	....	1·23	1— 8·0?	50,642	+90; − 0	20,626·5
7.	Argelander: DM.	....	....	324,198	....	15·19	1—10· ?	626,630	+90; − 2	21,344·7
8.	Celoria: Pub. Milan XIII.	....	27,216	200,781	....	93·12	1—11· ?	3,841,650	+ 6; 0	2,156·5
9.	Palisa: MS. and pub. (Pola) ...	628	....	62,774	....	99·96	1—13· ?	4,123,600	+28; −32	175±
10.	Palisa: MS. and pub. (Vienna)...	228	....	29,574	....	129·71	1—14·6?	5,350,900	+29; −25	94±
11.	Peters: MS. maps (only)	954	....	68,410	....	71·71	1—14·8?	2,958,200	+25; −26	950±
12.	Peters: published maps.........	500	....	43,928	....	87·87	1—14·9?	3,624,800	+20; −25	500±
13.	W. Herschel: Series I. (published)	683	3,400	27,989	40·98	827·46	1—15· ?	34,135,000	+46; −32	168·3
14.	W. Herschel: Series II. (unpublished)	405	2,245	21,964	54·23	1095·1	1—15· ?	45,174,000	+81; −32	111·2
15.	J. Herschel: C. G. H.	605	2,299	68,948	113·96	2301·2	1—15· ?	94,929,000	0; −90	113·9
16.	W. and J. Herschel: all observed	1,693	7,944	118,882	70·22	1417·9	1—15· ?	58,492,000	+81; −90	393·4

Table B.—*Statistics of Star-Gauges.*

Limiting R.A. 1860·0.	Argelander, DM. No. of stars per 1° 1–10 mags.	Palisa's Pola Maps.		Palisa's Vienna Maps.		Peters's MS. Maps.		W. Herschel's Gauges.		Herschel ÷ Argelander.	Herschel ÷ Palisa (Pola).	Herschel ÷ Palisa (Vienna).	Herschel ÷ Peters (MS.).
		No. of ¼ degrees counted.	o. of Stars per 1° 1–13? mags.	No. of ¼ degrees counted.	No. of stars per 1° 1–4·6? mags.	No. of ¼ degrees counted.	No. of stars per 1° 1–14·8 mags.	No. of ¼ degrees counted.	No. of stars per 1° 1–15? mags.				
h m h m													
0 0 to 0 20	15·9	29	76·0	13	104·8	45	53·7	12	240	15·1	3·2	2·3	4·5
0 20 to 0 40	15·9	1	61·0	......	......	......	......	8	481	30·3	? 7·9	......	......
0 40 to 1 0	15·7	......	......	......	......	40	62·0	12	459	29·2	......	......	7·4
1 0 to 1 20	15·3	......	......	......	......	50	71·0	21	313	20·5	......	......	4·4
1 20 to 1 40	15·9	......	......	10	93·0	15	60·3	9	436	27·4	......	4·7	7·2
1 40 to 2 0	15·5	9	80·9	36	87·9	12	72·5	9	166	10·7	2·1	1·9	2·3
2 0 to 2 20	15·2	15	67·9	3	90·0	......	......	9	160	10·5	2·4	1·8	......
2 20 to 2 40	14·7	53	84·2	......	......	......	......	16	487	33·1	5·8	......	......
2 40 to 3 0	13·3	24	80·0	......	......	......	......	10	198	14·9	2·5	......	......
3 0 to 3 20	13·5	8	56·5	19	136·3	25	86·8	8	129	9·6	2·3	0·9	1·5
3 20 to 3 40	13·4	17	86·8	5	128·0	......	......	12	267	19·9	3·1	2·1	......
3 40 to 4 0	13·1	14	112·6	......	......	......	......	11	285	21·8	2·5	......	......
4 0 to 4 20	12·7	9	102·0	......	......	25	70·5	11	164	12·9	1·6	......	2·3
4 20 to 4 40	12·9	16	108·5	......	......	......	......	15	287	22·2	2·6	......	......
4 40 to 5 0	16·3	8	108·1	......	......	25	123·0	10	372	22·8	3·4	......	3·0
5 0 to 5 20	18·7	1	157·0	......	......	......	......	11	709	37·9	4·5	......	......
5 20 to 5 40	21·1	......	......	......	......	......	......	14	739	35·0	......	......	......
5 40 to 6 0	21·6	6	204·8	......	......	......	......	28	1,182	54·7	5·8	......	......
6 0 to 6 20	22·6	......	......	......	......	......	......	34	1,420	62·8	......	......	......
6 20 to 6 40	22·8	4	294·5	......	......	......	......	28	1,295	56·8	4·4	......	......
6 40 to 7 0	22·0	8	341·4	......	......	......	......	36	1,683	76·5	4·9	......	......

7 0 to 7 20	18·9	......	......	......	......	......	......	25	1,810	95·8	......	......	......
7 20 to 7 40	16·8	6	185·0	......	......	......	......	25	1,974	117·5	10·7	......	......
7 40 to 8 0	15·6	17	183·4	10	187·0	2	130·5	17	1,921	123·1	10·5	10·3	14·7
8 0 to 8 20	14·4	8	182·0	25	224·5	......	......	12	1,380	95·9	7·6	6·1	......
8 20 to 8 40	13·2	......	......	......	......	16	80·8	6	652	49·4	......	......	8·1
8 40 to 9 0	12·2	2	107·0	......	......	16	67·5	15	382	31·3	3·6	......	5·7
9 0 to 9 20	11·2	16	70·8	......	......	20	62·7	6	444	39·6	6·3	......	7·1
9 20 to 9 40	10·4	57	80·1	19	80·2	16	62·0	7	291	27·9	3·6	3·6	4·7
9 40 to 10 0	10·1	9	107·7	3	98·7	21	66·3	11	236	23·4	2·2	2·4	3·6
10 0 to 10 20	9·7	25	91·0	25	104·4	25	76·7	4	222	22·9	2·4	2·1	2·9
10 20 to 10 40	9·0	7	82·3	......	......	15	55·3	9	143	15·9	1·7	......	2·6
10 40 to 11 0	9·2	1	44·0	......	......	15	72·2	4	232	25·2	? 5·3	......	3·2
11 0 to 11 20	8·5	20	77·9	6	97·8	45	54·8	6	156	18·4	2·0	1·6	2·8
11 20 to 11 40	8·8	9	72·0	......	......	36	58·3	7	133	15·1	1·9	......	2·3
11 40 to 12 0	8·3	......	......	......	......	44	57·5	3	135	10·5	......	......	2·3
12 0 to 12 20	8·4	......	......	3	121·3	75	59·8	4	121	14·4	......	1·0	2·0
12 20 to 12 40	8·8	......	......	31	123·5	5	58·6	6	145	16·5	......	1·2	2·5
12 40 to 13 0	8·7	......	......	3	86·3	4	59·5	5	133	15·3	......	1·5	2·2
13 0 to 13 20	8·5	9	92·2	......	......	......	......	8	133	15·7	1·4	......	......
13 20 to 13 40	8·9	19	68·4	......	......	5	70·0	10	152	17.1	2·2	......	2·2
13 40 to 14 0	8·9	3	64·7	......	......	25	74·8	9	168	18·9	2·6	......	2·2
14 0 to 14 20	9·4	......	......	......	......	25	83·6	11	202	21·5	......	......	2·4
14 20 to 14 40	9·4	......	......	......	......	......	......	8	192	20·4	......	......	......
14 40 to 15 0	9·6	......	......	......	......	......	......	12	202	21·1	......	......	......
15 0 to 15 20	9·6	......	......	......	......	......	......	10	166	17·3	......	......	......
15 20 to 15 40	10·0	......	......	......	......	5	96·8	19	206	20·6	......	......	2·1
15 40 to 16 0	10·7	5	65·0	......	......	5	102·2	17	295	27·6	4·5	......	2·9
16 0 to 16 20	11·7	24	90·4	......	......	......	......	19	180	15·4	2·0	......	......
16 20 to 16 40	12·0	......	......	......	......	......	......	17	331	27·5	......	......	......
16 40 to 17 0	12·9	......	......	......	......	......	......	11	432	33·5	......	......	......
17 0 to 17 20	13·3	......	......	......	......	......	......	3	537	40·4	......	......	......
17 20 to 17 40	16·0	......	......	......	......	......	......	9	497	31·0	......	......	......

TABLE B.—*continued.*

Limiting R.A. 1860·0.	Argelander, DM. No. of stars per 1° 1–10 mags.	Palisa's Pola Maps.		Palisa's Vienna Maps.		Peters's MS. Maps.		W. Herschel's Gauges.		Herschel ÷ Argelander.	Herschel ÷ Palisa (Pola).	Herschel ÷ Palisa (Vienna).	Herschel ÷ Peters (MS.).
		No. of ¼ degrees counted.	No. of stars per 1° 1–13? mags.	No. of ¼ degrees counted.	No. of stars per 1° 1–14·6? mags.	No. of ¼ degrees counted.	No. of stars per 1° 1–14·8 mags.	No. of ¼ degrees counted.	No. of stars per 1° 1–15? mags.				
17 40 to 18 0	17·4	....	....	13	231·2	....	....	12	1,081	62·1	......	4·7	......
18 0 to 18 20	20·3	5	199·0	1	157·0	....	....	20	1,563	77·0	7·9	10·0	......
18 20 to 18 40	21·6	....	....	....	....	....	....	36	1,018	47·1	......	......	......
18 40 to 19 0	22·9	....	....	....	....	....	....	31	2,266	97·2	......	......	......
19 0 to 19 20	23·5	6	222·2	....	....	10	200·3	23	2,772	113·7	12·0	......	13·4
19 20 to 19 40	26·6	....	....	1	263·0	....	....	42	4,547	170·9	......	17·3	......
19 40 to 20 0	25·5	....	....	....	....	....	....	37	4,153	151·0	......	......	......
20 0 to 20 20	25·8	....	....	....	....	5	132·4	33	2,165	83·9	......	......	16·4
20 20 to 20 40	23·7	4	213·0	....	....	5	97·0	25	1,370	57·8	6·4	......	14·1
20 40 to 21 0	22·0	4	155·0	....	....	5	78·2	27	1,927	87·6	12·4	......	24·7
21 0 to 21 20	20·6	....	....	....	....	25	81·4	23	1,505	73·0	......	......	18·5
21 20 to 21 40	19·8	....	....	....	....	49	91·0	15	964	48·7	......	......	10·6
21 40 to 22 0	18·7	8	92·5	2	119·5	31	86·7	15	1,376	73·6	14·9	11·5	15·9
22 0 to 22 20	18·7	34	111·1	....	....	50	76·8	16	503	26·9	4·5	......	6·6
22 20 to 22 40	18·2	36	29·8	....	....	25	65·0	12	176	9·7	1·9	......	2·7
22 40 to 23 0	17·4	29	81·8	....	....	25	59·8	13	297	17·1	3·6	......	5·0
23 0 to 23 20	16·1	27	82·4	....	....	17	66·1	17	214	13·3	2·6	......	3·2
23 20 to 23 40	16·1	5	120·0	....	....	30	53·7	11	131	8·1	1·1	......	2·4
23 40 to 24 0	15·5	11	57·2	....	....	20	62·1	10	234	15·1	4·1	......	3·8
Sums....................................		628	5113·1	228	2535·4	954	3280·3	1088	53,797				
Weighted means ...	15·19 (72)		116·2 (44)		133·4 (19)		80·1 (41)		747·2 (72)				

In Table A it is clear that the irregularity of the distribution of the stars in R.A. in the various series has influenced the numbers of col. 8 so that they are not comparable. The numbers for Palisa's 6-inch telescope are greater than those for Peters. The Table B gives similar data for certain of the authorities for every 20^m of R.A. throughout the 24^h. All the counts refer to 1860·0, except those of Argelander, which are given for 1855·0.

Column 1 gives the limits of R.A.; column 2 gives the average number of stars from the first to the tenth magnitude, in the 'Durchmusterung,' quoted from the Bonn observations, vol. 5; column 3 gives the number of counts of areas of 2^m in R.A. by 30′, made by Dr. Palisa on his Pola maps, and column 5 the same datum for his Vienna maps (these include both published and unpublished maps); column 4 gives the number of stars per 1° square derived from the Pola maps, and column 6 the same quantity for the Vienna maps; these numbers are the sum of the quantities 4 sec. δ times the *actual* number of stars, divided by the number of counts; column 7 gives the number of square degrees counted on Dr. Peters's manuscript maps (not including the very few of Professor Watson's); and column 8 gives the average number of stars per 1° square for Peters's derived as above; column 9 gives the number of gauges (not fields) counted by Sir W. Herschel within the R.A.'s noted; and column 10 gives the number of stars per 1° square for Herschel.

The total number of square degrees actually counted is :—

For Argelander, 21344·7;
For Palisa (Pola), 175 ±;
For Palisa (Vienna), 94 ±;
For Peters (MS.), 950 ±;
For W. Herschel (I. & II.), 279·5.

The limits of declination are given in Table A. I have not included Sir J. Herschel's gauges, as they refer to the southern hemisphere only, while the others refer partly to the northern sky.

The last four columns of the table give the ratios:

Number of stars per 1° (Herschel) divided by number of stars per 1° for Argelander, Palisa (Pola), Palisa (Vienna), and Peters, respectively.

These ratios show again how far from uniform is the scattering of the stars. While it is true that the problem of the true distribution of the stars, if it is to be solved at all, must be solved by neglecting the individual peculiarities of small regions, yet it appears to be evident from the table, that areas much smaller than 20^m in R.A. by 92° in declination must be considered. I have taken them so large in this table only because the counts for the 'Durchmusterung' are given to every 20^m.

It appears to me that one of the most important conclusions from these tables is, that the method of star-gauging must be applied to the study of comparatively small regions, and that the results from these are then to be combined into larger groups and so on.

The same region gauged with five different apertures, for example, will give more results of value than five times this area gauged with a single telescope. If these two tables serve to finally dispose of the fundamental assumption that the stars are equally scattered in space and to bring about the study of their distribution on a more general basis, they will have served a valuable end.

Washburn Observatory, University of Wisconsin, Madison, 1884, June 1. EDWARD S. HOLDEN.

Sir William Herschel's Observations of Variable Stars *.

THE discovery last summer of two additional catalogues of the Light of the Stars by William Herschel has been announced elsewhere. At the same time a Journal was found, which gives the dates of observation for the individual comparisons contained in the six catalogues. The suggestion was made by Mr. Chandler, that the observations of variable stars contained in these catalogues would thus be rendered of value. The observations contained in Table I. were kindly forwarded by Lieut.-Col. Herschel, who has taken much trouble in furnishing me with all the material available for the following discussion. The successive columns of Table I. give a current number, the number of the catalogue of Herschel in which the star is contained, the usual designation of the star, and its Flamsteed number. The next columns give the date, the observation, and the resulting magnitude. The latter is derived from the photometric observations made in 1879–82 at Harvard College Observatory with the meridian photometer, and will appear in the Annals of the Observatory, volume xiv. The values assumed for the intervals employed by Herschel are obtained from a discussion of all his catalogues, and will appear in the same volume; they are 0·1 for a period, 0·2 for a comma, and 0·4 for a dash.

The scale employed may be defined as that in which a magnitude corresponds to the ratio whose logarithm is four tenths, and which coincides with the scale of Argelander for the magnitude 5. It is best illustrated by the statement that the magnitudes 3, 4, 5, and 6 on the scales of the 'Uranometria Nova' and the 'Durchmusterung' would be expressed by 3·1, 4·2, 5·0, and 5·8 on the photometric scale.

* Proceedings of the American Academy of Arts and Sciences, 1884, p. 269.

TABLE I.

o.	Cat.	Name.	Fl.	Date.	Comparison.	Magn.
1.	I.	η Aquilæ	55	1795, July 19	65, 30, 55. 60	3·8
2.		"		" "	60. 55	4·1
3.		"		" "	55—41—38. 44	3·9
4.		"		1795, July 25	41, 55	4·5
5.	I.	*g* Herculis	30	1795, May 23	1, 30	4·7
6.		"		1795, May 25	30—25	5·1
7.		"		" "	11. 35—6, 30	4·9
8.		"		" "	6, 52, 30	5·2
9.		"		" "	52, 42, 30, 34	5·6
10.		"		1795, Sept. 20	30. 1	4·4
11.		"		" "	30—, 25	4·9
12.	I.	*u* Herculis	68	1795, May 22	71. 68, 72	5·2
13.		"		" "	68, 90, 72	5·1
14.		"		1795, Aug. 18	68, 59, 61	5·1
15.		"		" "	69—68	5·3
16.	II.	*o* Ceti	68	1779, Nov. 30	$\beta > o > a$	2·4
17.		"		1798, Feb. 18	68. 82	4·0
18.	II.	η Geminorum	7	1795, Mar. 29	μ—η—ξ''	3·3
19.		"		" "	μ—ε—η	3·6
20.		"		1795, Nov. 7	27; 13, 7—31	3·2
21.		"		1796, Feb. 1	13; 7	3·3
22.		"		" "	13. 7—, 31	3·1
23.		"		1796, Nov. 30	13—, 123 Tauri, 7	3·2
24.	II.	ζ Geminorum	43	1795, Nov. 7	55. 77, 34—43	4·1
25.	III.	δ Cephei	27	1796, Nov. 5	32, 27	3·8
26.	IV.	β Lyræ	10	1782, May 12		—
27.		"		1795, May 5		—
28.		"		1795, Sept. 15	14—, 10	3·8
29.	IV.	R Lyræ	13	1796, Aug. 28	12. 13	4·5
30.		"		" "	21. 20, 13	4·7
31.	N V.	ρ Persei	25	1795, Aug. 21	23, 25—41, 46	3·4
32.		"		1796, Sept. 7	45——39—25	3·6
33.	IV.	λ Tauri	35	1796, Jan. 1	1—2, 35. 38	4·0
34.		"		1796, Nov. 30	123—, 35	3·6
35.	V.	X Sagittarii	3	1795, Sept. 15	2, 3	—
36.	VI.	δ Libræ		1795, May 11	37, 31. 35, 44. 19. 43. 45. 6	5·2
37.		"		1795, May 18	37, 31, 19	5·4
38.		"		" "	7, 19	5·6
39.		"		1797, May 22	19. 18	5·9

The following remarks appear in the original record:—

No. 16. "*o* Ceti is less than β and larger than α. See p. 26."

No. 22. "Seems to be larger than it has been."

No. 26. "To the n-eye γ much larger than β. (Mem. This star is changeable, and was then at its minimum.)"

No. 27. "β Lyræ much less than γ, $10^h\ 45^m$ com. time."

No. 28. "10 seems to be at its minimum."

The variations of several of these stars are irregular, or at least the law governing them is not yet known. It is evident that these observations will hereafter form a most valuable test of the correctness of any assumed law, since in many cases they precede by more than half a century any other observations of these stars of the same degree of precision. This is shown in Table II. which gives in successive columns the name of the star, the number of observations contained in Table I., the year in which the variability was discovered, and the number of years by which this followed the observations of Herschel. A dash is inserted when the discovery preceded the observations. In these cases the observations of Herschel have less value, since we have contemporaneous or antecedent observations which serve to determine the nature of the changes. The last three columns give the period in days, and the magnitude at maximum and minimum, according to the catalogue of Professor Schönfeld *.

TABLE II.

Name of Star.	No. Obs.	Discov.	Years.	Period.	Variation. Schönfeld.	
η Aquilæ	4	1784	—	7·2	3·5	4·7
g Herculis	8	1857	62	irr.	5	6·2
u Herculis	5	1869	74	irr.	4·6	5·4
o Ceti	3	1596	—	331·3	1·7–5·0	8·9
η Geminorum	9	1865	70	229·1	3·2	3·7–4·2
ζ Geminorum	1	1844	49	10·2	3·7	4·5
δ Cephei	1	1784	—	5·3	3·7	4·9
β Lyræ	3	1784	—	12·8	3·4	4·5
R Lyræ	2	1856	60	46·0	4·3	4·6
ρ Persei	3	1854	59	irr.	3·4	4·2
λ Tauri	3	1848	52	3·9	3·4	4·2
X Sagittarii	1	1866	71	7·0	4	6
δ Libræ	5	1859	64	2·3	4·9	6·1

The individual stars will now be considered in turn, so far as the material exists for a more complete reduction than is given in Table I.

η *Aquilæ*.—The variation in light of this star has been discussed by Argelander †. A light curve is given by which the brightness at any time may be expressed in terms of an arbitrary scale of grades. This scale is defined by expressing in grades the light of the comparison stars used in determining the changes in brightness of the variable. Points have been constructed for each of these stars, with grades as abscissas, and the photometric magnitudes as ordinates. A straight line drawn nearly through these points serves to convert the scale of grades into photometric magnitudes, or into actual light ratios. From this we may conclude that the maximum and minimum light of the variable expressed in photometric magnitudes is 3·7 and 4·5. The range is accordingly less than that ordinarily given, but this is partly due to the difference in the scales. The observations of Herschel expressed in

* Zweiter Catalog von veränderlichen Sternen. Mannheim, 1875.

† Astron. Nach. xix. p. 399.

grades equal 9·6, 6·1, 8·5, and 1·0. The first three of these correspond to periods of $1^d\ 19^h$, $1^d\ 4^h$, and $1^d\ 14^h$ after a minimum, if the light of the star was increasing, or to $3^d\ 4^h$, $5^d\ 6^h$, and $3^d\ 14^h$, if the light was diminishing. The other observation, No. 4, which was made six days later, on July 25, serves to decide between these two hypotheses. The light was then sensibly that of a minimum at 1·2 grades. As the period of the star is about $7^d\ 4^h$, a minimum about a day preceding the observations 1, 2, 3 would also be indicated by observation 4. This would agree with the hypothesis that the light was increasing on July 19, but would controvert the view that it was diminishing.

The time of the observations is defined only by the limits of twilight, which in the latitude of Slough would extend to within about two hours of midnight in July. The star would culminate near midnight, and could be easily observed as long as darkness lasted. The times of observation may therefore be written, 1795, July $19^d\ 12^h \pm 2^h$, and 1795, July $25^d\ 12^h \pm 2^h$. The mean of the three results on July 19 would give an interval from the minimum of $1^d\ 12^h$, or would place the preceding minimum at 1795, July $18^d\ 0^h$, with an uncertainty of several hours, since a small error in the light corresponds to a large deviation in the time of minimum. The elements of Argelander indicate a minimum 1795, July 18^d $19^h\ 42^m$, Paris M.T. EDWARD C. PICKERING.

[To be continued.]

CORRESPONDENCE.

To the Editor of 'The Observatory.'

Meteors of July 23–25 *and August* 10, 1884.

SIR,—

On the nights of July 23–25, 1884, I observed 72 shooting-stars. The most conspicuous shower was from a well-defined point between α and β Persei, at α 48°, δ 43°+. Fifteen meteors were referable to this stream, and they were similar in their visible traits to the August Perseids, being rather bright, and the phosphorescent streaks which are almost uniformly generated by the swift meteors from radiants near the apex of the Earth's way, were also, in the case of this shower, freely exhibited, especially with the brighter meteors.

I have seen this same shower of July Perseids in several preceding years, but never with such distinctness before. I call attention to it, partly from the fact that it shows a close agreement with the radiant-point of Messier's comet of 1764 (49° +45°·5, July 25), and may have some derivative connection with it, though the assumption is somewhat doubtful, as the comet, at the descending node, passed some ten millions of miles inside the Earth's orbit. The analogy seems worth mentioning, however, for little is known as to the possible dispersion of cometary particles in the plane of their orbits, owing to the repelling power of the Sun, which may originate considerable width in the resulting meteor-streams. The suggestion has been thrown out (I believe by Weiss and Schiaparelli) that a comet during its circumsolar passage, and when the tail is wheeled rapidly round in conformity with the phenomena usually observed, may radiate a portion of its material over a wide space in the direction of increasing perihelion distance, giving a marked difference of orbit to the particles composing different parts of the stream. Thus the Earth probably encounters many fragments belonging to these distended cometary systems, though the circumstances may not altogether appear to favour such an inference. The orbits of comets are computed from the paths of the nuclei of these bodies; but these orbits must differ considerably from the orbits of such of the materials as are scattered over the much larger spaces swept by the envelopes, tail, and other appendages.

The meteor-shower near α-β Persei evidently affords a bright display preceding the true Perseids, and it will be necessary to reobserve it and determine the date of its greatest intensity. July 20–21 is a somewhat notable fireball epoch, and it is very probable that this special radiant has supplied many of the more prominent meteors which have been recorded at this time.

My recent observations enabled me to recognize another rich shower in the extreme N.W. boundary of Capricornus near γ. I recorded 9 very slow meteors from a radiant at $301° -15°$. I had noticed this stream on July 28, 1878, at $305° -15°$, on July 6–17, 1877, at $298° -8°$, and on Aug. 6-9, 1880, at $300° -10°$. It has also been well determined by Schmidt, Tupman, Konkoly, and Sawyer. I find the mean centre from 12 good radiants is at $304°\cdot7 -8°\cdot5$. Mr. Greg's average position (1876) is at $299° -7°$, from 6 radiants extending over the interval from June 1 to July 25. Both Neumayer and Tupman have observed the shower in June. It is most conspicuous, however, and has been far more generally noticed, during the last half of July, when it provides many brilliant, slow-moving meteors.

Of the other showers seen in July this year, none were of any prominence; but on the 23rd I recorded several very long meteors from a radiant at $75° +32°$ on the horizon. On the 24–25th I saw 3 Aquarids, with paths averaging some 40°, the point of radiation in this case also being extremely low. On July 25 I noticed the first indications of the true Perseids. Clouds prevented work on the later nights of the month.

On August 10 bright moonlight and an otherwise unfavourable state of sky greatly obscured the Perseids. There were bands of white filmy cloud stretching in a north and south direction, and not many stars were visible. Between $9\frac{1}{2}^h$ and $11\frac{1}{2}^h$, however, I observed 16 bright Perseids from a radiant not very exactly determined at $44° +59°$. Under the circumstances it was impossible to judge as to the character of the display this year.

Bristol, 1884, August 12. W. F. Denning.

The Eclipse of Pericles.

SIR,—

In his letter to you on this subject, Mr. Johnson still contends for the eclipse of March 30th, B.C. 433, rather than that of August 3rd, B.C. 431, as the one in question (which occurred in the first year of the Peloponnesian war), on the ground that Thucydides was too accurate a writer to say that several stars appeared (*ἀστέρων τινῶν ἐκφανέντων*) if the obscuration was only sufficient (and barely sufficient) to bring out the planet Venus. I must confess this seems to me a literal clinging to his words, almost like the *αὐτὸς ἔφη* with which we are told the disciples of Pythagoras repudiated any calling in question what their master said. I think I can call to mind many instances in which it has been said to me, "The stars are out!" when in the dusk of evening twilight one star only has actually been seen. It is to be remembered that, in any case, a star or stars can only be seen momentarily during an eclipse. Nor is it even likely that more stars were visible at the eclipse of B.C. 433.

But, however that be, there are two arguments which, unless I can see them rebutted, will continue to lead me to give the preference to that of B.C. 431.

One of these (which I mentioned in my previous letter, but do not see that Mr. Johnson has noticed) is the number of events that took place in the campaign before the eclipse, which could not have occurred before March 30, and point rather to such a date as August 3.

The other is mentioned by Mr. Johnson himself (p. 18 of his 'Eclipses Past and Future') as a difficulty, and seems to me to be a fatal one. Another eclipse of the Sun is mentioned by Thucydides (book iv. c. 52), which appears to have occurred in the eighth year of the war. He calls it a small eclipse (*τοῦ τε ἡλίου ἐκλιπές τι ἐγένετο*), and says that it took place at the beginning of the summer (as before, it would seem that he uses the word *θέρος* as almost equivalent to the period of time which would be covered by the campaign). There seems to be little or no doubt that this was the eclipse of the 21st of March, B.C. 424; and if this was the eighth year of the war, B.C. 431 must have been the first.

Mr. Johnson refers to a small pamphlet published in 1852 by W. Drew Snooke, formerly of Ryde, Isle of Wight, in which the author gives the places of Venus and of several of the brightest stars at the time of the eclipse of August 3, B.C. 431. It does not appear, however, that Mr. Johnson has verified Snooke's result, which makes Venus only "a little eastward of the Sun." An approximate calculation made by myself indicates that she was not far from greatest eastern elongation, and therefore *considerably* to the east of the Sun. At any rate, there can be, I think, little doubt that the planet would be visible during the eclipse (although partial), especially in the atmosphere of Greece, and with the Sun low in the heavens.

Yours faithfully,

W. T. LYNN.

Blackheath, 1884, Aug. 16.

Polar Flattening of Venus.

SIR,—

At p. 239 of your number for August I find that the measures of Venus on the French Transit of Venus photographs show a polar flattening of $\frac{1}{303}$. I have also seen somewhere an account of measurements of Venus recently showing a flattening in the N.–S. direction, of which my unsettled state has caused me to lose the reference. I should be greatly obliged if you or your readers could supply it.

The matter is one of some interest, especially to me. In 1874, from measures taken during the Transit of Venus, I found a compression in the N.–S. direction of $\frac{1}{259\cdot3 \pm 77\cdot6}$*, and I might fairly have assigned even a higher probable error, because this value depends on the probable errors of the arc values of the measures in the two directions, whereas the difference of the micrometer readings is far more certain. If two other perfectly independent sets of measures now show, as they seem to do, the same sort of result, we have a strong presumption that there is a real flattening in the N.–S. direction. This, however, does not seem consistent with the determinations of the inclination of the equator of Venus to the ecliptic which have been received hitherto, and it seems desirable to examine the question again.

I am, yours truly,

37 Hamilton Road, Ealing, W. J. F. TENNANT, Lieut.-Gen.
1884, August 1.

Saturn.

SIR,—

In looking through the old volumes of the 'Monthly Notices of the R.A.S.,' I have just come upon a letter from Mr. Lassell, in which he speaks of seeing the ball of the planet Saturn through the division between the rings, precisely as Professor Hall and myself saw it last November, with the Princeton telescope.

The letter is dated Valetta, Nov. 1, 1852, and will be found in Vol. xiii. The passage (on p. 12) reads as follows :—

"There is evidently also a sudden paleness of the main division of the rings where it crosses the ball—scarcely sufficiently marked in the engraving—as if, in some degree at least, *the ball were seen through the division.*"

The same thing appears in Mr. De la Rue's figure, opposite p. 30; but, from a remark in the note on p. 24, I judge that Mr. De la Rue did not see it himself.

Princeton, New Jersey, U.S.A. C. A. YOUNG.
Aug. 4, 1884.

* Monthly Notices R.A.S. Vol. xxxv. p. 345. The records are at Greenwich.

The August Meteors.

Sir,—

I think it will be well to send a short report to you of the Perseid meteor-shower this year; for although I saw nothing whatever worth recording, yet the comparative absence of meteors was of itself an interesting fact, which should certainly be chronicled, if we are to find out any thing about returns of maxima and minima.

Since I have carefully watched this shower I have never seen such a poor display. Of course the full Moon and some light cloud made a great difference; but it was obvious that there was but little to see, for, of course, the large meteors would have been observable had there been any.

In watches, including a few hours on the 10th, 11th, and 12th &c. of August, I registered 62 meteors, 47 being Perseids.

The hourly number of Perseids never got above 10 for one observer, even when for a little while on the 11th it was almost dark and very clear.

There were so few first mag. ones that the horary number, if it had been clear and dark on the 10th, could not, by comparison with the usual allowance of the larger meteors, have been more than 15 or 20. There can be little doubt that this return has been one of the very feeblest for some years. Yours truly,

Great Baddow, 1884, Aug. 23. H. Corder.

NOTES.

Variations in the Solar Diameter.—Dr. J. Hilfiker, Assistant Astronomer at the Observatory of Neuchâtel, has recently published a little pamphlet, in which he discusses the question of the variability of the solar diameter. For this purpose he has reduced a series of 3468 transits of the Sun observed during the last 22 years at the Neuchâtel Observatory. These were all observed with the same instrument, a circle by Ertel fils, with an object-glass by Merz of 115 mm. aperture and 2 m. focal length. Each limb of the Sun was observed on 13 threads, and a magnifying-power of 200 was employed.

Dr. Hilfiker refers to several suggestions which have been made in order to explain the different values obtained for the solar diameter at different times in the year, such as the effect of differences of temperature on the telescope, the different coloured glasses employed, or a supposed ellipticity of the Sun, the polar diameter being the longer, but does not throw much additional light upon them, nor does he arrive at any very definite conclusions. But with regard to the apparent variations in the mean annual diameter of the Sun, Dr. Hilfker is more decided, and he concludes (1) that the variations in the value of the diameter of the Sun indicated by the Neuchâtel observations are real; and (2) that these changes bear a relation to the period of the sun-spots, that is to say that the greatest diameters coincide with the minimum of the period of sun-spots and *vice versâ*. The data, however, seem scarcely sufficient to justify the latter conclusion, which does not agree with those formed on the same subject by Auwers and by Newcomb.

OBSERVATORIES.—The article "Observatory" in the ninth edition of the 'Encyclopædia Britannica,' is from the pen of Dr. J. L. E. Dreyer, Director of Armagh Observatory, and formerly editor of 'Copernicus.' The article commences with a brief introduction giving an outline of the development of observatories, from the days of Hipparchus to the present time. A gazetteer of the principal existing observatories follows, in which is given a description of the equipment and work of each, as full as space permits. A number of English private observatories, now discontinued, are also described, on account of their historical interest and the important work which has been performed at many of them.

NEPTUNE.—In No. 2600 of the 'Astronomische Nachrichten' Dr. G. Müller gives the results of a number of observations of the brightness of Neptune made by him at the astrophysical observatory of Potsdam. The observations were made in four series, viz. 1878, Sept. 22 to Dec. 13; 1881, Sept. 20, to 1882, Jan. 15; 1883, Jan. 4 to Feb. 19; and 1883, Oct. 7 to Dec. 30. The planet was not observed on the dates on which Mr. Maxwell Hall ('Observatory,' No. 83, p. 72) observed the variation in its brightness which led him to attempt to determine its rotation period; but Dr. Müller's observations afford no indication of any such variability as that which Mr. Hall believed he had established. The mean results for each series are as follows:—

Series.	No. of Nights.	Comparison Star.	Magnitude of Neptune.
			m m
I.	11	DM. + 13° 411	7·97 ± 0·01
II.	17	DM. 13° 411	8·00 ± 0·02
III.	5	DM. 15° 430	7·83 ± 0·04
IV.	7	DM. 16° 432	8·00 ± 0·02

The separate observations of the last series are as follows:—

Potsdam Mean Time.	Magnitude of Neptune.	Difference from Mean.	Interval from Maximum.
h m	m	m	h
1883, Oct. 7, 9 52	7·87	− 0·13	3·9
13, 8 31	8·08	+ 0·08	3·9
Nov. 10, 6 38	8·08	+ 0·08	5·5
18, 9 48	7·95	− 0·05	1·8
24, 7 3	7·97	− 0·03	3·1
27, 9 5	7·95	− 0·05	0·4
Dec. 30, 6 1	8·07	+ 0·07	3·5

The last column gives the interval from maximum, assuming Mr. Hall's date of Dec. 2^d $15^h.3$ G. M. T. and period of $7^h.92$. The observations were made with a Zollner photometer of Ausfeld's construction in the first three series, and in the last series with one by Wanschaff. The Grubb refractor of 8-in. aperture was used in the first and third, and the Steinheil of 5·3-in aperture in the second and fourth series.

Stonyhurst College Observatory.—From the "Results" of observations made at this Observatory in 1883 we learn that the usual magnetic and meteorological observations have been continued as in former years, and that the solar drawings and spectroscopic observations of the chromosphere, prominences, and sun-spots have become part of the regular daily work. In 1883, drawings of the Sun on the scale of 10½ in. to diameter were made on 180 days, the chromosphere was completely examined on 72 days and partially on 9, and the spectra of sun-spots on 16 days. In addition to various magnetic and meteorological tables, observations of auroræ are given for April 3 and 24, May 11 and 14, August 30, September 8, October 4 and 5. A careful collation of these observations with the sun-pictures and magnetic curves suggests the following remarks:—"Auroral displays coincide in every instance with periods of solar disturbance, and there have as yet been no auroræ remarked during periods of solar quiet. Similarly, in every case of auroræ the magnets were disturbed, although the disturbances synchronous with the auroræ of May 11, August 30, and September 8 were not of a violent character. The auroræ of April and May coincide fairly with a large group of spots which appeared on the following limb of the Sun on April 11, and was followed until nearly the end of May. Similarly the auroral displays of August and October synchronize with a spot which was followed from August 29 until early in October. The intermediate aurora of September 8 may have been connected with a large spot first seen on September 9, and which later became a scattered group and died out about November 13. We may also remark that the displacements of the bright lines in the spectrum of the chromosphere favour the supposed connection between auroræ and solar disturbance. Thus the observations of April 2 and 25 showed considerable displacements of the C line, especially on the former date. There is also some evidence to show that the auroræ and magnetic storms synchronize rather with particular classes of spots than with solar disturbances generally." Observations taken at St. Ignatius' College, Malta, form an appendix to the Stonyhurst Results for 1883.

The Zodiacal Light.—The current volume of the 'Proceedings of the American Academy of Arts and Sciences' contains a very valuable paper by Mr. A. Searle on the Zodiacal Light, in which he has collected and reduced on a uniform system the evening observations of the principal observers of the zodiacal light. The points taken up are the approximate position of the zodiacal cone in the visible hemisphere of the sky, the elongation of the vertex, and the latitudes of the northern and southern boundaries at successive elongations 30° apart. The details of more than 650 observations by Jones, Heis, Schmidt, and others are exhibited in tabular form, whilst their results are conveniently and completely summarized in a number of other tables showing the monthly means, and means for different series.

Mr. Searle supports Jones's view that the apparent changes in the place of the light should be referred rather to the corresponding changes in the place of the ecliptic in the visible hemisphere than to the geographical position of the observer in latitude, and regards it as probable that atmospheric absorption is an important and perhaps the only cause of the variations of the zodiacal light in latitude. But "if atmospheric absorption has the importance here assigned to it, in the study of the zodiacal light, we cannot expect to determine the true position of the light on any occasion by the simple methods heretofore in use." Direct photometric observations must be made, or, failing these, observers "must compare together different portions of the light and also specified portions of the light and of the Milky Way." And the Milky Way must itself be studied in a similar systematic manner. A careful photometric inquiry "is indispensable if we are to substitute definite knowledge for the vague information now before us with regard to 'zodiacal bands,' the singular phenomenon of the 'Gegenschein,' and the possibly periodical variations in the main body of the zodiacal light, as well as its apparent changes from hour to hour."

In dealing with this question of the photometric observation of the light, Mr. Searle mentions the interesting fact that from Celoria's and Sir W. Herschel's observations the Milky Way would appear to be about two magnitudes brighter than the mean brightness of the sky. On this estimate the brighter parts of the zodiacal light would be commonly three or four magnitudes brighter than the surrounding sky.

Mr. Searle remarks in conclusion :—"It is not my intention, on this occasion, to dicuss the probability of any explanation of the zodiacal light. I have merely to remark with regard to the ordinary meteoric theory, that it gains greatly in simplicity if we dispense with all the imaginary meteoric bodies or rings with which it has usually been connected and retain merely the conception of meteoric dust diffused throughout the Solar System. It may be shown mathematically, if we regard the meteoric particles as solids reflecting light irregularly, that an appearance like the zodiacal cone with an indefinite vertex would result."

A NEW SUNSHINE-RECORDER*.—Being doubtful of the accuracy of the published sunshine records, Mr. Herbert M'Leod endeavoured, during the summer of 1880, to devise an apparatus by which the

* Phil. Mag. 1884 Aug., p. 141.

light instead of the heat of the Sun would produce the record of sunshine. The results of his experiments were communicated to the Physical Society on 1884, June 28. The apparatus consists of a camera placed with its axis parallel to the polar axis of the Earth, the lens pointing northwards. Opposite the lens is a silvered sphere from which the Sun's rays are reflected through the lens on to ferro-prussiate paper, thus forming a distorted image of the Sun. The positions of the lens and sphere are so arranged that the image is linear and radial, and is carried round in a circular arc by the motion of the Earth. The sphere is a chemical round-bottomed flask about 95 millim. in diameter, silvered inside; the distance from flask to lens is 76 millim., and from lens to paper 152 millim.; the lens has a focal length of 90 millim. and an effective aperture of 22 millim. The circle traced in June is about 120 millim. in diameter. An impression is obtained with an exposure of only 10 seconds, and when the lens is covered for one minute a light line is produced in the circular band, so that the paper is sufficiently sensitive in the present apparatus to register short gleams of sunshine and also the passage of small clouds. When the Sun shines through light clouds, the impression produced is somewhat blurred and of a much less intense blue than when the Sun is unobscured. The time-scale is made by drawing radial lines from the centre of the circular band, containing angles of 15°, each of which represents one hour. It still remains to be found what are the most convenient dimensions for globe and lens, so that the minimum of alteration of position will be requisite to obtain a sharp image at all times of the year, and also the best method of fixing the paper so that it may be easily changed and the time-scale marked on it.

Cincinnati Observatory.—We learn from the 'Kansas City Review' that Prof. J. G. Porter of the U.S. Coast Survey has been appointed to succeed Prof. Ormond Stone as director of the Cincinnati University Observatory, Prof. Stone having accepted the position of Professor of Astronomy in the University of Virginia more than a year ago. The Cincinnati Observatory has been superintended in the interim by Mr. H. C. Wilson, who has been appointed Assistant Astronomer. Prof. Porter was at one time an assistant at the Litchfield Observatory, Hamilton College, Clinton, New York, and now holds an important position in the U.S. Coast Survey.

Remarkable Meteor-train *.—Mr. Brooks of Phelps, N. Y., records an observation of a remarkable meteor-train, which he saw about $8^h\ 30^m$ on July 3. The meteor moved from near Polaris towards the N.W., but had disappeared when Mr. Brooks's attention was attracted to that part of the sky. He describes the train as "The most brilliant and wonderful it has been my privilege to witness. Notwithstanding the bright twilight glow of that early hour the train of light stood out bold and distinct thereon, resembling a

* Sidereal Messenger, 1884 Aug., p. 167.

brilliant naked-eye comet, for which for a moment it could have been easily mistaken. At first it was quite straight, but in 3 or 4 minutes it began to curve in different directions, still maintaining its distinct outlines. Then nuclei or points of condensation would form in various parts of the train, which slowly expanded and diffused until they became invisible. These nuclei were distinctly seen by the aid of a small glass, and I regret that, on account of being about half a mile from the observatory at the time, I was not able to turn the large reflector upon this very interesting phenomenon. The train of light was visible to the naked eye fully ten minutes, and in a good telescope could have been seen much longer. I have in several instances while comet-sweeping, ran upon a curious twisting mass of meteoric smoke or débris quite invisible to the naked eye, but never anything equalling this in brilliancy and variety of detail."

COMET (*b*) 1884.—Mr. Barnard has communicated the following particulars relative to the discovery of Comet (*b*) to the 'Sidereal Messenger':—"On the night of July 16, while sweeping in the S.W. portion of my zone, I discovered a suspicious-looking object, which from its absence from my memory and the catalogues at once suggested the probability of its being a comet. A series of pointings was begun with the 6-in. equatoreal upon the object and neighbouring stars. The mean of four careful pointings gave its place at $9^h 33^m 55^s$ Nashville M.T., R.A. $15^h 50^m 40^s$, South Dec. $37° 9' 52''$. It was close *n.f.* a star of 8m. (?), which star I noticed was of a reddish tinge. The next night was cloudy. On the 18th the sky was clear for a short time and the object was examined again. It was located precisely the same with respect to a star which I called 8 or 9 m.; the declination was the same. I at once concluded it was a nebula, but thought it brighter than on the 16th. The sky quickly clouded. On the 19th the sky was badly clouded, but, through breaks, I glanced at the object and saw at once it had moved. I had been deceived the night before by the striking similarity of comet and star; I described it as brighter, but, as it was seen for a few seconds only, may have been deceived. The 20th being clear, the comet was again observed and described as fainter. It is large for a telescopic comet, gradually a little brighter in the middle. On the last date I suspected an indefinite rapid brightening to a nucleus near the middle, possibly *f* the centre. It spreads out into hazy indeterminate outlines. Its motion is very slow towards the east, some 20′ daily, with an almost insensible southern motion."

M. Perrotin describes this comet as having, on Aug. 15, the appearance of a somewhat ill-defined nebulosity, about 1′ 30″ in diameter, and presenting bright granulations towards its centre.

THE SYSTEMATIC SEARCH FOR COMETS.—The systematic search for comets organized by the 'Science Observer' has been carried on with considerable zeal, the northern zones having been swept on an average of two to six times per month during the year ending March last; but observers who can command from 15° to 45° S. declination are still needed.

A NEW FAINT NEBULA *.—Mr. Barnard, of Nashville, Tenn., announces the discovery of a new nebula which appears as a small faint object in a 6-in. telescope, the light being of an even tint. The mean position for 1884·0 is α 17^h 16^m $45^s{\cdot}8$, δ $-38°$ $26'$ $18''$, or 2^m 22^s f and $3'{\cdot}9$ south of Gen. Cat. 4290. There is a faint star a little south, which confuses the light of the nebula. Herschel's description of 4290 is "!!!; ⊙; *eF*; *S*; *am St*"; but Mr. Barnard could not make out its annular character.

Mr. J. E. GORE, M.R.I.A., has prepared a catalogue of known variable stars, with copious notes, which the author has taken pains to render accurate and useful. The legend of the identity of Tycho Brahé's star with the "stars" of 945 and 1264, and with the star of the Magi, is, however, again repeated here. The catalogue was read as a paper before the Royal Irish Academy 1884, January 28.

WE have received Vol. iv. (new series) of the 'Annals' of the Royal Observatory, Brussels. It contains the observations with the transit-instruments made in 1879–80 and 1881; phenomena of Jupiter's satellites, 1880–81; physical observations of Jupiter, 1879–81, with copies of 140 drawings made by M. L. Niesten, who has a justly high reputation as an astronomical artist; observations of Comets *b* and *c*, 1881, with 16 drawings by the same observer; 32 drawings of parts of the Moon's surface, with notes by M. Stuyvaert, and a valuable series of drawings of the solar spectrum by M. C. Fievez. We have already referred to M. Fievez's work on former occasions ('Observatory,' No. 64, p. 242, and No. 78, p. 308). This is the first time spectroscopic observations have appeared in the 'Annals' of this Observatory, and the Director regrets that it is at present impossible to devote more attention to this interesting branch of astronomy.

A NEW Observatory has recently been erected at Doane College, Crete, Nebraska, under the supervision of Prof. G. D. Swezey. The instruments already secured are a Buff and Burger Transit, an 8-inch equatoreal by Alvan Clark and Sons, a Howard mean-time and a Seth-Thomas clock, a Sewell break-circuit chronometer, and a set of meteorological instruments. The observatory will be known as Boswell Observatory †.

A NEW Minor Planet (No. 239) was discovered by Herr Palisa at Vienna on Aug. 18. It was of the 13th magnitude. This makes Herr Palisa's 43rd discovery. Prof. C. H. Peters has discovered 42.

THE Minor Planet No. 238 has received the name of Hypatia.

* Sidereal Messenger, 1884 Aug., p. 184. † Ibid. p. 190.

Maxima and Minima of Variable Stars in 1884, September.

M, signifies maximum ; *m*, minimum.

Sept.	2·0	ζ Geminorum, M.		
	2·7	β Lyræ, *m*.		
	4	R Virginis, M.		
	6	S Ursæ Maj., M.		
	6	R Herculis, M.		
	7	R Boötis, M.		
	7·1	ζ Geminorum, *m*.		
	7·3	W Virginis, M.		
	9	η Geminorum, M.		
	10	R Cancri, M.		
	12	R Ursæ Maj., M.		
	12·2	ζ Geminorum, M.		
	13	U Monocerotis, M.		
	13	T Aquarii, M.		
	13	R Vulpeculæ, M.		
	14	R Sagittæ, *m*.		
	14·6	T Monocerotis, *m*.		
Sept.	15	S Cephei, *m*.		
	15·6	β Lyræ, *m*.		
	16	R Piscium, M.		
	16·3	W Virginis, *m*.		
	17	S Vulpeculæ, *m*.		
	17·3	ζ Geminorum, *m*.		
	20	S Libræ, *m*.		
	22	R Lyræ, *m*.		
	22·3	ζ Geminorum, M.		
	22·5	T Monocerotis, M.		
	24	R Lacertæ, M.		
	24·6	W Virginis, M.		
	25	R Coronæ, *m*.		
	27·4	ζ Geminorum, *m*.		
	28·5	β Lyræ, *m*.		
	29	R Persei, M.		
	30	R Scorpii, M.		

Variables of Short Period.

δ Cephei.

	d.
Sept.	4·85, *m*.
	6·45, M.
	11·75, M.
	15·55, *m*.
	17·15, M.
	22·55, M.
	26·25, *m*.
	27·85, M.

η Aquilæ.

	d.
Sept.	3·5, M.
	8·3, *m*.
	10·7, M.
	15·5, *m*.
	17·9, M.
	22·7, *m*.
	29·8, *m*.

X Sagittarii.

	d.
Sept.	1·6, *m*.
	4·5, M.
	8·6, *m*.
	11·5, M.
	15·6, *m*.
	18·5, M.
	22·6, *m*.
	25·5, M.
	29·6, *m*.

U Sagittarii.

	d.
Sept.	2·3, *m*.
	5·3, M.
	22·5, *m*.
	25·5, M.
	29·3, *m*.

W Sagittarii.

	d.
Sept.	2·5, *m*.
	5·7, M.
	13·3, M.
	17·7, *m*.
	25·3, *m*.
	28·5, M.

U Cephei, *m*.

		h	m
Sept.	3	10	47
	8	10	27
	13	10	6
	18	9	46
	23	9	25
	28	9	5

Algol, *m*.

		h	m
Sept.	1	16	34
	4	13	23
	7	10	12
	10	7	1
	21	18	17
	24	15	6
	27	11	54
	30	8	43

S Cancri, *m*.

		h	m
Sept.	12	10	40

λ Tauri, *m*.

		h	m
Sept.	2	17	55
	6	16	47
	10	15	39
	14	14	32
	18	13	24
	22	12	16
	26	11	8
	30	10	1

δ Libræ, *m*.

		h	m
Sept.	3	12	33
	10	12	7
	17	11	41
	24	11	15

U Coronæ, *m*.

		h	m
Sept.	2	10	25
	9	8	7
	16	5	50
	19	16	41
	26	14	24

Elements and Ephemeris of Comet 1884 *b* (*Barnard*).
By Carl Stechert *.

Perihelion Passage 1884 Aug. 18·21985, Berlin M.T.

ω	305° 51′ 8″·4	Mean Equinox 1884·0.
☊	357 40 19 ·0	
i	6 52 11 ·8	
log *q*	0·140670	

Ephemeris for Berlin Midnight.

1884.	R.A. h m	Dec. ° ′	Log Δ.	Brightness.
Sept. 11....	19 5·3	−30 41	9·8325	0·83
12....	19 9·4	30 22		
13....	19 13·4	30 3		
14....	19 17·4	29 43		
15....	19 21·4	29 23	9·8470	0·76
16....	19 25·4	29 3		
17....	19 29·3	28 43		
18....	19 33·2	28 22		
19....	19 37·1	28 1	9·8629	0·69
20....	19 40·9	27 40		
21....	19 44·7	27 19		
22....	19 48·5	26 57		
23....	19 52·2	26 35	9·8801	0·62
24....	19 55·9	26 14		
25....	19 59·5	25 52		
26....	20 3·1	25 30		
27....	20 6·7	−25 8	9·8985	0·55

Differences of Right Ascension and Declination between Iapetus and the Centre of Saturn. By A. Marth †.

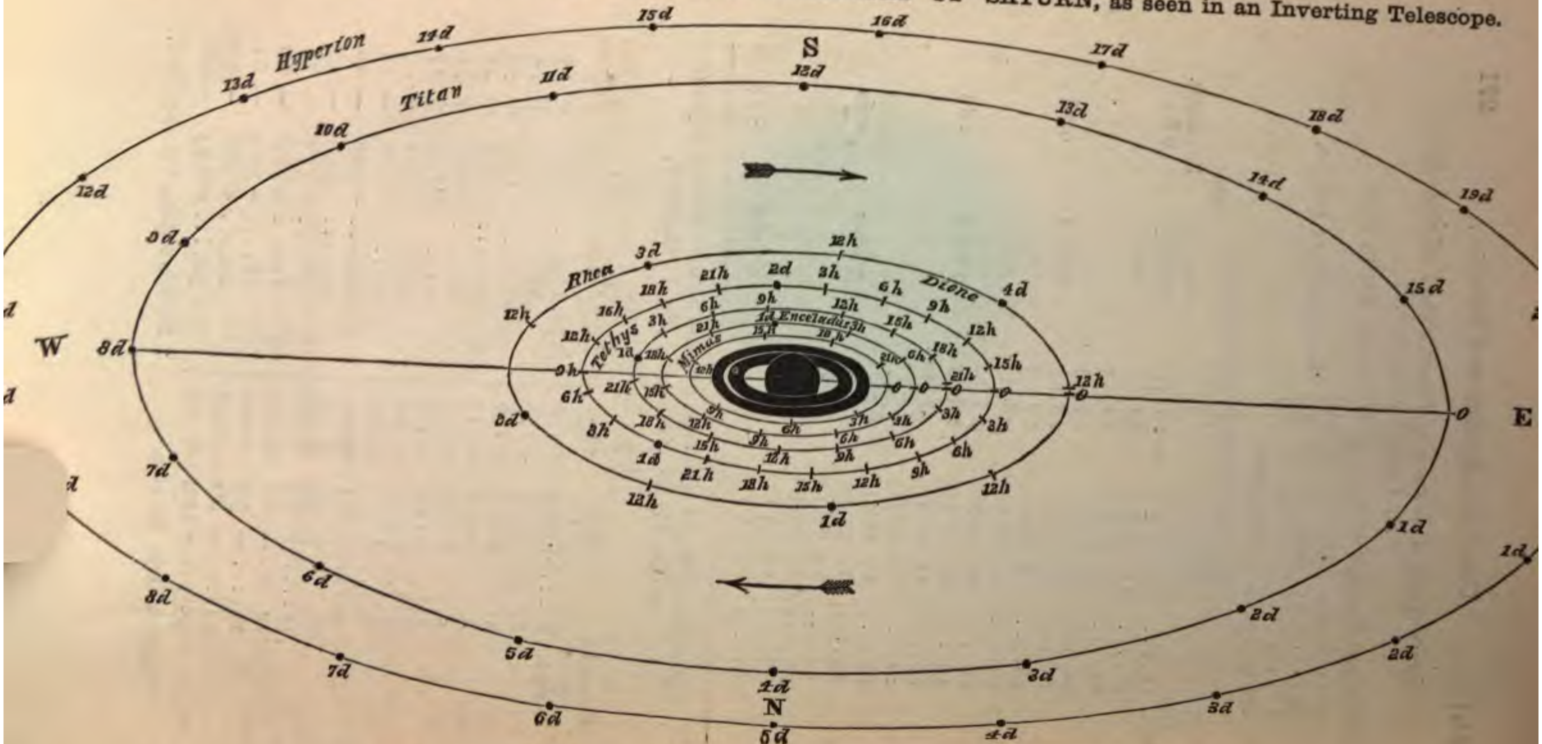
Japetus − Saturn.

Greenwich Noon.	R.A. s	Dec. ″	Greenwich Noon.	R.A. s	Dec. ″
Sept. 1....	+26·9	+176	Sept. 16....	−14·3	+ 85
2....	+24·7	+177	17....	−17·0	+ 72
3....	+22·5	+176	18....	−19·7	+ 59
4....	+20·1	+175	19....	−22·3	+ 45
5....	+17·5	+173	20....	−24·7	+ 31
6....	+14·9	+169	21....	−27·0	+ 17
7....	+12·2	+165	22....	−29·1	+ 3
8....	+ 9·3	+160	23....	−31·1	− 12
9....	+ 6·4	+153	24....	−32·8	− 26
10....	+ 3·5	+146	25....	−34·3	− 40
11....	+ 0·5	+138	26....	−35·5	− 54
12....	− 2·5	+129	27....	−36·6	− 68
13....	− 5·5	+119	28....	−37·4	− 81
14....	− 8·5	+108	29....	−38·0	− 94
15....	−11·4	+ 97	30....	−38·3	−106

* Astron. Nachr. No. 2609.

† Monthly Notices, Vol. xliv. No. 8.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East elongation.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elonga-
n, the times of those elongations, both East and West, which
visible at Greenwich are given.

MAS(East).		h
pt.	13	16·3
	14	14·9
	15	13·5
	16	12·1
	29	16·8
	30	15·4

MAS(West).		
pt.	21	16·5
	22	15·1
	23	13·7
	24	12·3

YPERION.		
pt.	5	14·4
	26	16·6

ENCELADUS.		h
Sept.	1	7·8
	2	16·7
	4	1·6
	5	10·5
	6	19·4
	8	4·3
	9	13·2
	10	22·1
	12	7·0
	13	15·9
	15	0·8
	16	9·7
	17	18·6
	19	3·5
	20	12·4
	21	21·3
	23	6·2
	24	15·1
	26	0·0
	27	8·8
	28	17·7
	30	2·6

TETHYS.		h
Sept.	1	18·2
	3	15·5
	5	12·8
	7	10·1
	9	7·4
	11	4·7
	13	2·0
	14	23·3
	16	20·6
	18	17·9
	20	15·2
	22	12·5
	24	9·8
	26	7·2
	28	4·5

TITAN.		
Aug.	29	13·6
Sept.	14	14·8
	30	15·0

DIONE.		h
Sept.	2	22·7
	5	16·4
	8	10·1
	11	3·8
	13	21·6
	16	15·3
	19	9·0
	22	2·7
	24	20·5
	27	14·2
	30	7·9

RHEA.		
Sept.	3	19·2
	8	7·6
	12	20·1
	17	8·5
	21	21·0
	26	9·4
	30	21·8

The following table will furnish the means for inferring roughly
e distances and position-angles of the satellites from the diagram
ven on the opposite page.

	Position-angle of Minor Axis of Ring.		Semi-major axis of orbit of Titan.
ug. 31	354° 22′	Aug. 31	182″·5
ept. 10	354 18	Sept. 10	185 ·8
20	354 16	20	189 ·2
30	354 15	30	192 ·8

Astronomical Memoranda, 1884, *September.*

Equation of Time :—Sun *before* Clock, Sept. 1, 0^m 17^s; Sept. 11, 39^s; Sept. 21, 7^m 10^s; Sept. 30, 10^m 11^s.
Sidereal Time at Mean Noon :—Sept. 1, 10^h 44^m 13^s; Sept. 11, ^{h} 23^m 38^s; Sept. 21, 12^h 3^m 4^s; Sept. 30, 12^h 38^m 33^s.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Sept. 3	17 18	6 40	22 1	+7 15	119 31
8	17 27	6 30	23 8	7 15	53 29
13	17 35	6 19	24 6	7 12	347 28
18	17 42	6 7	24 55	7 6	281 29
23	17 51	5 56	25 34	6 56	215 30
28	18 0	5 46	26 3	+6 44	149 30

The position-angles of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon. The position-angle of the Sun's axis is the position-angle of the N. end of the axis, from the N. point of the Sun, read in the direction N., E., S., W. In computing the heliographic latitude of the centre of the Sun's disk, the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74°. In computing the heliographic longitude of the centre of the disk, the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the prime meridian.

Moon.	*sets.* h m		*rises.* h m		*sets.* h m
Sept. 1	13 51	Sept. 11	9 53	Sept. 21	6 48
2	14 56	12	10 48	22	7 16
3	16 4	13	11 50	23	7 47
4	17 15	14	12 58	24	8 22
5	*rises*	15	14 9	25	9 2
6	6 52	16	15 21	26	9 48
7	7 20	17	16 32	27	10 40
8	7 51	18	17 43	28	11 37
9	8 25	19	*sets*	29	12 39
10	9 5	20	6 22	30	13 45

Full Moon, Sept. 4, $22^h\ 56^m$; Last Quarter, Sept. 11, $20^h\ 17^m$; New Moon, Sept. 18, $21^h\ 37^m$; First Quarter, Sept. 26, $22^h\ 21^m$.

Mercury, stationary Sept. $5^d\ 14^h$ and $27^d\ 12^h$; in inferior conjunction with the Sun Sept. $19^d\ 3^h$.

Venus, at greatest elongation (46° 5′ W.) Sept. $21^d\ 0^h$. Diameter:—Sept. 1, 29″·6; Sept. 30, 21″·2. Illuminated portion of disk 0·473 on Sept. 15.

	R.A.	Dec.	tr.	rises
Sept. 1,	$7^h\ 45^m·1$,	17° 11′ N.,	$21^h\ 0^m$,	$13^h\ 26^m$
30,	9 35 ·8,	13 18 N.,	20 57	13 46

Mars an evening star in Virgo and during last week in Libra, in conjunction with the Moon Sept. $22^d\ 0^h$. Diameter:—Sept. 1, 5″·2; Sept. 30, 4″·9. Illuminated portion of disk 0·956 on Sept. 15.

	R.A.	Dec.	tr.	sets
Sept. 1,	$13^h\ 23^m·3$,	8° 46′ S.,	$2^h\ 39^m$,	$7^h\ 58^m$
30,	14 37 ·9,	15 46 S.,	1 59	6 41

Jupiter in Leo, in conjunction with the Moon Sept. 16^d 14^h. Diameter:—Sept. 1, 29″·3; Sept. 30, 30″·4.

Sept. 1,	R.A. 9^h 34^m·3,	Dec. 15° 10′ N.,	tr. 22^h 47^m,	rises 15^h 25^m
30,	9 57 ·3,	13 17 N.,	21 16	14 3

Saturn, between Taurus and Gemini, in conjunction with Moon Sept. 12^d 2^h, in quadrature with the Sun Sept. 15^d 22^h.

Sept. 1,	R.A. 5^h 30^m·6,	Dec. 21° 51′ N.,	tr. 18^h 43^m,	rises 10^h 35^m
30,	5 35 ·1,	21 52 N.,	16 54	8 49

	Outer Ring.		Inner Ring.		Ball.
	Maj. Axis.	Min. Axis.	Maj. Axis.	Min. Axis.	Diam.
Sept. 10......	41″·30	18″·48	27″·46	12″·29	16″·4
30......	42 85	19 14	28 49	12 73	17 0

The south side of the rings is visible, the elevation of the Earth above their plane on Sept. 10 being 26° 35′ S., on Sept. 30, 26° 32′ S., and of the Sun 26° 44′ S. and 26° 45′ S.

Uranus in conjunction with the Sun Sept. 20^d 16^h.

Neptune.

Sept. 1,	R.A. 3^h 25^m·6,	Dec. 16° 54′ N.,	tr. 16^h 39^m,	rises 9^h 6^m
30,	3 24 ·4,	16 48 N.,	14 43	7 10

Phenomena.

	G. M. T. h m			G. M. T. h m	
Sept. 5	12 4	11 Piscium Oc. D. 120°.	Sept. 16	16 43	J. i. Oc. R.
5	15 1	14 Piscium Oc. D. 120°.	22	16 36	J. iii. Tr. E.
11	16 9	J. iii. Ec. D.	23	15 40	J. i. Ec. D.
12	11 14	B.A.C. 1930 Oc. R. 257°.	25	5 28	29 Ophiuchi Oc. D. 117°.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image. EDITOR.

Publications received:—J. M. Schaberle, *Lateral Astronomical Refraction* (Amer. Journ. Science, vol. xxvii., 1884, June)—H. Seeliger, *Ueber die Gestalt des Planeten Uranus* (Sitzungsberichten der math.-phys. Classe der k. bayer. Akad. d. Wiss. 1884, Heft 2)—T. Hilfiker, *Première Etude sur les Observations du diamètre du Soleil* (Bulletin de la Société des Sciences Naturelles, Neuchâtel, Tome xiv.)—A. Belopolsky, *Essai d'une détermination du rayon apparent du Soleil au moyen de la photographie* (Moscow Observations, 1883)—J. L. E. Dreyer, *Article "Observatory"* (Encyclopædia Britannica, Ninth Edition)—W. Valentiner, *Veroffentlichungen der Grossherzoglichen Sternwarte zu Karlsruhe.*

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Pending Problems of Astronomy *.

WITH your permission, I propose this evening to consider some of the pending problems of astronomy,—those which seem to be most pressing, and most urgently require solution as a condition of advance; and those which appear in themselves most interesting, or likely to be fruitful, from a philosophic point of view.

Taking first those that lie nearest, we have the questions which relate to the dimensions and figure of the Earth, the uniformity of its diurnal rotation, and the constancy of its poles and axis.

I think the impression prevails that we already know the Earth's dimensions with an accuracy even greater than that required by any *astronomical* demands. I certainly had that impression myself not long ago, and was a little startled on being told by the superintendent of our Nautical Almanac that the remaining uncertainty was still sufficient to produce serious embarrassment in the reduction and comparison of certain lunar observations. The length of the line joining, say, the Naval Observatory at Washington with the Royal Observatory at the Cape of Good Hope is doubtful—not to the extent of only a few hundred feet, as commonly supposed; but the uncertainty amounts to some thousands of feet, and may possibly be a mile or more, probably not less than a ten-thousandth of the whole distance; and the *direction* of the line is uncertain in about the same degree. Of course, on those portions of either continent which have been directly connected with each other by geodetic triangulations, no corresponding uncertainty obtains; and as time goes on, and these surveys are extended, the form and dimensions of each continuous land-surface will become more and more perfectly determined. But at present we have no satisfactory means of obtaining the desired accuracy in the relative position of places separated by oceans, so that they cannot be connected by chains of triangulation. Astronomical determinations

* Address to the American Association for the Advancement of Science at Philadelphia, Sept. 5, 1884, by Prof. C. A. YOUNG, Professor of Astronomy at Princeton, retiring President of the Association.

of latitude and longitude do not meet the case; since, in the last analysis, they only give at any selected station the *direction of gravity* relative to the axis of the Earth, and some fixed meridian plane, and do not furnish any *linear* measurement or dimension.

Of course, if the surface of the Earth were an exact spheroid, and if there were no irregular attractions due to mountains and valleys and the varying density of strata, the difficulty could be easily evaded: but, as the matter stands, it looks as if nothing short of a complete geodetic triangulation of the whole Earth would ever answer the purpose,—a triangulation covering Asia and Africa, as well as Europe, and brought into America by way of Siberia and Behring Strait.

It is indeed theoretically possible, and just conceivable, that the problem may some day be reversed, and that the geodesist may come to owe some of his most important data to the observers of the lunar motions. When the relative position of two or more remote observatories shall have been precisely determined by triangulation (for instance, Greenwich, Madras, and the Cape of Good Hope), and when, by improved methods and observations made at these fundamental stations, the Moon's position and motion relative to them shall have been determined with an accuracy much exceeding anything now attainable, then by similar observations, made simultaneously at any station in this hemisphere, it will be theoretically possible to determine the position of this station, and so, by way of the Moon, to bridge the ocean, and ascertain how other stations are related to those which were taken as primary. I do not, of course, mean to imply that, in the *present state* of observational astronomy, any such procedure would lead to results of much value; but, before the Asiatic triangulation meets the American at Behring Strait, it is not unlikely that the accuracy of lunar observations will be greatly increased.

The present uncertainty as to the Earth's dimensions is not, however, a sensible embarrassment to astronomers, except in dealing with the Moon, especially in attempting to employ observations made at remote and ocean-separated stations for the determination of her parallax.

As to the form of the Earth, it seems pretty evident that before long it will be wise to give up further attempts to determine exactly what spheroid or ellipsoid *most nearly corresponds* to the actual figure of the Earth; since every new continental survey will require a modification of the elements of this spheroid in order to take account of the new data. It will be better to assume some closely approximate spheroid *as a finality*; its elements to be for ever retained unchanged, while the deviations of the actual surface from this ideal standard will be the subject of continued investigation and measurement.

A more important and anxious question of the modern astronomer is, Is the Earth's rotation uniform, and, if not, in what way and to what extent does it vary? The importance, of course, lies in the fact that this rotation furnishes our fundamental measure and unit of time.

Up to a comparatively recent date there has not been reason to suspect this unit of any variation sufficient to be detected by human observation. It has long been perceived, of course, that any changes in the Earth's form and dimensions must alter the length of the day. The displacement of the surface or strata by earthquakes or by more gradual elevation and subsidence, the transportation of matter towards or from the equator by rivers or ocean-currents, the accumulation or removal of ice in the polar regions or on mountain-tops,—any such causes must necessarily produce a real effect. So, also, must the friction of tides and trade-winds. But it has been supposed that these effects were so minute, and to such an extent mutually compensatory, as to be quite beyond the reach of observation; nor is it yet certain that they are not. All that can be said is, that it is now beginning to be *questionable* whether they are or are not.

The reason for suspecting perceptible variation in the Earth's revolution lies mainly in certain unexplained irregularities in the apparent motions of the Moon. She alone, of all the heavenly bodies, changes her place in the sky so rapidly, that minute inaccuracies of a second or two in the time of observation would lead to sensible discrepancies in the observed position; an error of one second, in the time, corresponding to about half a second in her place,—a quantity minute, certainly, but perfectly observable. No other heavenly body has an apparent movement anywhere nearly as rapid, excepting only the inner satellite of Mars; and this body is so minute that its accurate observation is impracticable, except with the largest telescopes, and at the time when Mars is unusually near the Earth.

Now, of late, the motions of the Moon have been very carefully investigated, both theoretically and observationally; and, in spite of everything, there remain discrepancies which defy explanation. We are compelled to admit one of three things,—either the lunar theory is in some degree mathematically incomplete, and fails to represent accurately the gravitational action of the Earth and Sun, and other known heavenly bodies, upon her movements; or some unknown force other than the gravitational attractions of these bodies is operating in the case; or else, finally, the Earth's rotational motion is more or less irregular, and so affects the time-reckoning, and confounds prediction.

If the last is really the case, it is in some sense a most discouraging fact, necessarily putting a limit to the accuracy of all prediction, unless some other unchanging and convenient measure of time shall be found to replace the 'day' and 'second.'

The question at once presents itself, How can the constancy of the day be tested? The lunar motions furnish grounds of suspicion, but nothing more, since it is at least as likely that the mathematical theory is minutely incorrect or incomplete as that the day is sensibly variable.

Up to the present time, the most effective tests suggested are from the transits of Mercury and from the eclipses of Jupiter's satellites. On the whole, the result of Professor Newcomb's elaborate and exhaustive investigation of all the observed transits, together with all the available eclipses and occultations of stars, tends rather to establish the sensible constancy of the day, and to make it pretty certain (to use his own language) that "inequalities in the lunar motions, not accounted for by the theory of gravitation, really exist, and in such a way that the mean motion of the Moon between 1800 and 1875 was really less (*i. e.* slower) than between 1720 and 1800." Until lately, the observations of Jupiter's satellites have not been made with sufficient accuracy to be of any use in settling so delicate a question; but at present the observation of their eclipses is being carried on at Cambridge, Mass., and elsewhere, by methods that promise a great increase of accuracy over anything preceding. Of course, no *speedy* solution of the problem is possible through such observations, and their result will not be so free from mathematical complications as desirable,—complications arising from the mutual action of the satellites and the ellipsoidal form of the planet. On account of its freedom from all sensible disturbances, the remote and lonely satellite of Neptune may possibly some time contribute useful data to the problem.

We have not time, and it lies outside my present scope, to discuss whether, and, if so, how, it may be possible to find a unit of time (and length) which shall be independent of the Earth's conditions and dimensions (free from all *local considerations*), cosmical, and as applicable in the planetary system of the remotest star as in our own. At present we can postpone its consideration; but the time must unquestionably come when the accuracy of scientific observation will be so far increased, that the irregularities of the Earth's rotation, produced by the causes alluded to a few minutes ago, will protrude and become intolerable. Then a new unit of time will have to be found for scientific purposes, founded, perhaps, as has been already suggested by many physicists, upon the vibrations or motion of light, or upon some other physical action which pervades the universe.

Another problem of terrestrial astronomy relates to the constancy of the position of the Earth's axis in the globe. Just as displacements of matter upon the surface or in the interior of the Earth would produce changes in the time of rotation, so also would they cause corresponding alterations in the position of the axis and in the places of the poles,—changes certainly very minute. The only question is, whether they are so minute as to defy detection. It is easy to see that any such displacements of the Earth's axis will be indicated by changes in the *latitudes* of our observatories. If, for instance, the pole were moved a hundred feet from its present position, towards the continent of Europe, the latitudes of European observatories would be increased about one second, while in Asia and America the effects would be trifling.

The only observational evidence of such movements of the pole, which thus far amounts to anything, is found in the results obtained by Nyrén in reducing the determinations of the latitude of Pulkowa, made with the great vertical circle, during the last twenty-five years. They seem to show a slow, steady diminution of the latitude of this observatory, amounting to about a second in a century; as if the north pole were drifting away, and increasing its distance from Pulkowa at the rate of about one foot a year.

The Greenwich and Paris observations do not show any such result; but they are not conclusive, on account of the difference of longitude, to say nothing of their inferior precision. The question is certainly a doubtful one; but it is considered of so much importance, that, at the meeting of the International Geodetic Association in Rome last year, a resolution was adopted recommending observations specially designed to settle it. The plan of Signor Fergola, who introduced the resolution, is to select pairs of stations, having nearly the same latitude, but differing widely in longitude, and to determine the *difference* of their latitudes by observations of the same set of stars, observed with similar instruments, in the same manner, and reduced by the same methods and formulæ. So far as possible, the same observers are to be retained through a series of years, and are frequently to exchange stations when practicable, so as to eliminate personal equations. The main difficulty of the problem lies, of course, in the minuteness of the effect to be detected; and the only hope of success lies in the most scrupulous care and precision in all the operations involved.

Other problems, relating to the rigidity of the Earth and its internal constitution and temperature, have, indeed, astronomical bearings, and may be reached to some extent by astronomical methods and considerations; but they lie on the border of our science, and time forbids anything more than their mere mention here.

If we consider, next, the problems set us by the Moon, we find them numerous, important, and difficult. A portion of them are purely mathematical, relating to her orbital motion; while others are physical, and have to do with her surface, atmosphere, heat, &c.

As has been already intimated, the lunar theory is not in a satisfactory state. I do not mean, of course, that the Moon's deviations from the predicted path are gross and palpable,—such, for instance, as could be perceived by the unaided eye (this I say for the benefit of those who otherwise might not understand how small a matter sets astronomers to grumbling); but they are large enough to be easily observable, and even obtrusive, amounting to several seconds of arc, or miles of space. As we have seen, the attempt to account for them by the irregularity of the Earth's rotation has apparently failed; and we are driven to the conclusion, either that other forces than gravitation are operative upon the lunar motions, or else (what is far more probable, considering the past history of theoretical astronomy) that the mathematical theory is somewhere at fault.

To one looking at the matter a little from the outside, it seems as if that which is most needed just now, in order to secure the advance of science in many directions, is a new, more comprehensive, and more manageable solution of the fundamental equations of motion under attraction. Far be it from me to cry out against those mathematicians who delight themselves in transcendental and n-dimensional space, and revel in the theory of numbers,—we all know how unexpectedly discoveries and new ideas belonging to one field of science find use and application in widely different regions: but I own I feel much more interest in the study of the theory of functions and differential equations, and expect more aid for astronomy from it.

The problem of any number of bodies moving under their mutual attraction, according to the Newtonian laws, stands, from a physical point of view, on precisely the same footing as that of *two* bodies. Given the masses, and the positions and velocities corresponding to any moment of time, then the whole configuration of the system for all time, past and future (abstracting outside forces, of course), is absolutely determinate, and amenable to calculation. But while, in the case of *two* bodies, the calculation is easy and feasible, by methods known for two hundred years, our analysis has not yet mastered the general problem for more than two. In special instances, by computations, tedious, indirect, and approximate, we can, indeed, carry our predictions forward over long periods, or indicate past conditions with any required degree of accuracy; but a general and universally practicable solution is yet wanting. The difficulties in the way are purely mathematical; a step needs to be taken, corresponding in importance to the introduction of the circular functions, into trigonometry, the invention of logarithms, or the discovery of the calculus. The problem confronts the astronomer on a hundred different roads; and, until it is overcome, progress in these directions must be slow and painful. One could not truly say, perhaps, that the lunar theory must, in the meanwhile, remain quite at a standstill: labour expended in the old ways, upon the extension and development of existing methods, may not be fruitless, and may, perhaps, after a while, effect the reconcilement of prediction and observation far beyond the present limits of accuracy. But if we only had the mathematical powers we long for, then progress would be as by wings; we should fly, where now we crawl.

As to the physical problems presented by the Moon, the questions relating to the light and heat—the radiant energy—it sends us, and to its temperature, seem to be the most attractive at present, especially for the reason that the results of the most recent investigators seem partially to contradict those obtained by their predecessors some years ago. It now looks as if we should have to admit that nearly all we receive from the Moon is simply *reflected* sun-light and sun-heat, and that the temperature of the lunar surface nowhere rises as high as the freezing-point of water, or even of mercury. At the same time, some astronomers of reputation are not disposed to admit such an upsetting of long-received ideas; and it is quite certain that, in the course of the next few years, the subject will be carefully and variously investigated.

Closely connected with this is the problem of a lunar atmosphere,—if, indeed, she has any.

Then there is the very interesting discussion concerning changes upon the Moon's surface. Considering the difference between our modern telescopes and those employed fifty or a hundred years ago, I think it still far from certain that the differences between the representations of earlier and later observers necessarily imply any real alterations. But they, no doubt, render it considerably *probable* that such alterations have occurred, and are still in progress; and they justify a persistent, careful, minute, and thorough study of the details of the lunar surface with powerful instruments: especially do they inculcate the value of large-scale photographs, which can be preserved for future comparison as unimpeachable witnesses.

I will not leave the Moon without a word in respect to the remarkable speculations of Professor George Darwin concerning the tidal evolution of our satellite. Without necessarily admitting all the numerical results as to her age and her past and future history, one may certainly say that he has given a most plausible and satisfactory explanation of the manner in which the present state of things might have come about through the operation of causes known and recognized, has opened a new field of research, and shown the way to new dominions. The introduction of the doctrine of the conservation of energy, as a means of establishing the conditions of motion and configuration in an astronomical system, is a very important step. C. A. Young.

[To be continued.]

Markree Observatory.

Edward Joshua Cooper, the founder of Markree Observatory, was born at St. Stephen's Green, Dublin, in 1798 May. On the death of his grandfather two years later, the estate of Markree was inherited by the eldest son, whom ill-health prevented living there, the management being left in the hands of a younger brother, father of the founder, who from boyhood had exhibited a lively interest in astronomy. After spending some years in the endowed school of Armagh and Eton, young Cooper entered Christ Church College, Oxford, where he remained only two years, leaving without taking his degree. His next years were spent in travelling, always taking portable instruments, with which he determined the geographical position of many places. After spending part of the summer of 1820 in assisting Sir W. Drummond in obtaining copies of the ceilings of Denderah and Esneh at Naples, he engaged a competent Italian artist, and proceeded to Egypt as far as the Second Cataract of the Nile, the results of the journey being published in 1824, under the title "Views of Egypt and Nubia taken during the winter of 1820–21." Some of the letterpress to the lithographs is rather dry and devoid of much interest; but it must be remembered that Mr. Cooper was then a very young man, and too much interested in other sciences to find time for deep study of Eastern languages or Egyptology, which latter was then only in its infancy. In 1824–5 he travelled through Denmark, Sweden, and Norway, as far as the North Cape. The result of his travelling experience pointed to Nice and Munich as the most favourable places for observing; indeed, when visiting Nice twenty years later, he took the glasses of the great Markree reflector and used them mounted on a wooden stand, furnished with vertical and horizontal telescope movements. On this occasion he entered into communication with the Mayor of Nice, with a view to getting an observatory established there; but though the project was favourably discussed, no action was taken till lately, when M. Bischofsheim, the enlightened benefactor of French science, founded, endowed, and handed over to the Government of the Republic, a first class observatory.

Mr. Cooper began to keep meteorological registers at Markree in 1824; but owing to his frequent absence they were very imperfect until 1833, from which time till his death in 1863 they were as good and complete as the state of science at that time admitted. The rainfall, and to some extent the temperature registers, have been presented to the Meteorological Society, and I had intended to have published 5-day means for the series, but was unable to complete the work. In 1828 determinations of time &c. were made with the portable instruments; but, becoming manager of the property on the death of his father in 1830, Mr. Cooper at once took steps towards founding an observatory, intending to ensure its permanent activity by endowment. The prohibitive duties imposed on the manufacture of the pure glass required for the manufacture of achromatic object-glasses prevented the pursuit of studies tending to perfect the discovery of the principle of the achromatic object-glass. In 1824, Fraunhofer, one of the greatest men of science Germany has produced, having found out the secret, constructed an instrument of 9 inches aperture, which eclipsed all previous instruments and remained for some time the largest in the world. Some years later, Cauchoix, of Paris, made an object-glass 12 inches in diameter; owing to several circumstances this maker never received the credit he deserved, none of his object-glasses being retained in France, whose astronomical history bears traces of the loss up to the present day. Sir James South fully recognizing its excellence, secured the first of his make, and

Troughton, the distinguished but somewhat over self-confident optician, undertook to mount it equatorially, but did not succeed, as at some distance from the meridian it was very shaky. Sir J. South, after an expensive lawsuit with Troughton, scarcely ever used the instrument, and subsequently bequeathed it to Trinity College, Dublin, and it is now the principle instrument of the Dunsink Observatory. Hearing, in 1831, that Cauchoix had finished an object-glass of over 13 inches in diameter, which was greatly admired by Arago, Gambart, and other Parisian astronomers of that day, Mr Cooper at once bought and mounted it provisionally on a wooden stand at Markree. In the same year a transit instrument, 5 feet long, with an indifferent object-glass of 5 inches by Tully, was purchased from Troughton, and erected in the Eastern wing of the observatory; this instrument was never used in any work of scientific value, and was finally removed from the observatory in 1874. A model of a mural circle was also sent to Mr. Cooper, which was not approved of.

Meanwhile the report soon spread that in a remote corner of Ireland the largest telescope ever made had been erected by a gentleman then unknown to astronomical fame. Dr. Robinson, director of the Armagh Observatory, entered into communication with and visited Mr. Cooper, thus commencing a friendship which has since borne ample fruit for astronomy, and to which in a great measure Markree Observatory owes its position; for Dr. Robinson's intimate acquaintance with the principles of practical astronomy proved invaluable to Mr. Cooper in its equipment. Mr. Cooper's first idea was, as explained by Dr. Robinson, in a letter to the Editor of the 'Astronomische Nachrichten,' that the great telescope would be the means of adding considerably to our knowledge of double stars. It was found, however, that the lenses were not properly centred, the stars at times, instead of appearing as luminous points, having a ray of light emanating from them; difficulty was experienced in following the stars without an equatoreal stand, and the micrometer of course altered its position with respect to the equator when the telescope was out of the meridian. Thus the original idea was abandoned. The opinion of Dr. Robinson on the telescope was expressed in these words—"I have been no little amused at the insignificant appearance in my 10-feet reflector of some of the objects which were so very striking at Markree." In correspondence with Sir J. Herschel, reference was made to the merits of the Markree instrument and Sir John's 25-feet reflector — Sir John in 1833 stating that he could perceive a greater number of stars in M. 65 and the Orion nebula, but admitted the superiority of the definition in the refractor. Mr. Cooper found about this time (1833) that he had been using too high a power on the nebulæ; for though, as stated in text-books, the number of stars perceptible in an achromatic telescope increases with the magnifying-power, there is a limit above which the number diminishes with an increase of power.

Dr. Robinson, while endeavouring to effect a pacific solution of the dispute between South and Troughton, was convinced of grave defects in the form of mounting adopted by the latter; and having become acquainted with Mr. T. Grubb, then a clerk in a Dublin house, the question was discussed between them, the result being that Mr. Grubb soon satisfied Dr. Robinson of his capability of effectually mounting a large equatoreal, should opportunity offer. The opportunity soon offered, for Mr. Cooper, though not without some misgiving as to the result, acted upon the advice of Dr. Robinson and ordered an equatoreal mounting from Mr. Grubb. In December, 1832, the declination axis and circles were finished, and the polar axis was being cast. The maker did not fail to secure stability, Dr. Robinson's opinion being that "all were jolly-looking affairs, with an appearance of strength that seems to set unsteadiness at defiance." It was erected in 1834 on a foundation built of limestone blocks; both tube and stand are of cast-iron and weigh upwards of two tons. Dr. Robinson, in 1833, asked the Archbishop of Armagh to furnish him with a duplicate of this mounting for the 10-inch mirror; the request was granted in 1835, a 15-inch mirror being delivered by Grubb at the same time. As no maker could undertake the construction of a dome of the dimensions required for so large a telescope, a rough shelter was erected to protect the instrument as much as possible from the effects of changeable climate. Under date 1835, Dec. 8, Sir G. Airy wrote that he had launched a dome 27 feet in diameter, and thought it quite possible to have one of 36 feet diameter made for Markree; nevertheless the instrument still remains exposed to the open sky (the object-glass, when not in use, being covered by a metal cap, over which is a leather one). A wall 16 feet high serves to protect the observer from the wind, which, however, is free to act with full force on the upper half of the telescope; the yard in which it is placed is very damp. The driving-clock, though rude in comparison to modern instruments, deserved to be looked upon as a masterpiece at the time of its erection.

While in Dublin, the object-glass narrowly escaped a fatal accident, a couple of splinters being knocked out of its edge; the places were filled with pitch, which still remains; there are also some very fine scratches on it and a few veins, but the amount of light lost thereby is imperceptible, and such trifling defects are met with in even the best modern glasses.

Like many a young and enthusiastic amateur, Mr. Cooper soon tired of this telescope, and was desirous of obtaining one still larger—a desire which still actuates some astronomers of the present day, who forget the remark of Bessel, that a practical astronomer ought to be able to do something, even if he has only a cart-wheel and a gun-barrel at his disposal. At that time Daguet was experimenting with a view to producing disks for making object-glasses of 24 inches in diameter, and offered to produce a pair in two years for 28,000 francs, the price being reduced 1000 francs for every inch less diameter required, which, curiously enough, would leave 4000 francs as the price of a glass of no diameter at all! In 1838, Dr. Robinson informed Mr. Cooper that Grubb was casting a 9-inch mirror in zones, for the purpose of gaining experience to enable him to grind larger surfaces, but advised him to await the report on the large object-glass then being made by Merz for Pulkova. Herschel had told him that 10 feet was the limit for a reflector; but Arago thought, on unknown grounds, that 20 inches must ever remain the limit for an achromatic.

A rumour being current, in the beginning of 1839, that the Admiralty intended sending a large telescope to the Cape Observatory, Dr. Robinson advised Mr. Cooper to offer to sell his object-glass for the purpose, as he was anxious that the Markree telescope should continue to be the largest in the world. It was fortunate that Mr. Cooper did not order the gigantic mural circle of which Troughton had sent him the model, for the German meridian-circle was far superior. Being in Bavaria in 1839, he visited the works of Ertel, then the best maker of this class of instruments, where he met the Struvés, who were arranging for the instruments for the new observatory at Pulkova. The meridian-circle for the Glasgow Observatory was being constructed by Ertel, and Mr. Cooper, satisfied that the form of instrument was good, ordered one of larger dimensions than any previously made, the diameter of the object-glass being 7 inches; this instrument was brought to Ireland and erected at Markree by Ertel, junior. There were two circles 3 feet in diameter and each cast in one piece—one finely divided and read by eight microscopes; the other roughly for setting was acted on by the clamp. As it was not thought advisable to determine the error of collimation or the zenith-point by reversion of so large an instrument, it would be necessary to remove one of the circles to perform this operation. So far as I know, the instrument has never been reversed, although it can be done with some little trouble *; and as the object-glass and eyepiece can be interchanged, it is possible to observe stars with the instrument in four independent positions (not taking account of observations by reflexion *sub polo*). This instrument soon became out of date compared with those constructed a short time later by Repsold of Hamburg; some parts of it, however, are very fine; but it is a well-known fact that Ertel was occasionally very careless, and whether the pivots were ever good or the errors of division small, it would be now impossible to ascertain. Acting on the advice of Dr. Robinson, Mr. Cooper did not have the collimators supplied by Ertel with his instruments, but ordered them of Mr. Grubb; they were not erected till twenty years later. These collimators are fixed on piers touching the walls of the house on the outside, and,

* Mr. Graham states that the instrument was reversed once, but that the reversing apparatus seemed so unstable when the weight was on it, that Mr. Cooper would never venture to repeat the operation.

as the cube is not pierced, both object-glass and eyepiece have to be removed when they are used, which was fatal to the accuracy of the observation, and, indeed, no reliance was ever placed upon it.

W. Doberck,
Government Astronomer.
Hong-Kong Observatory.

[To be continued.]

Sir William Herschel's Observations of Variable Stars.

[Continued from p. 259.]

g Herculis.—The variations in light of this star are irregular, or the law governing them has not yet been discovered. Should this law ever be determined, these observations may have great value, since they anticipate by sixty-two years the first observations of equal accuracy previously known. Probably the variations are so slow that the hour at which the observations were made will not be needed.

u Herculis.—The same remark applies to this star as to the preceding. The observations are accordant, and anticipate other similar observations by seventy-four years.

o Ceti.—Some other observations by Sir William Herschel are given by Argelander *. As this star had been observed for many years previously, these observations are not of especial importance.

η Geminorum.—These observations precede by seventy years those taken elsewhere. They will therefore have great value in determining the period when the nature of the variations is more accurately established. The small change in light, however, makes the result derived from any small number of observations somewhat doubtful.

ζ Geminorum.—A comparison of the results obtained by Argelander † with the photometric measures gives the variation in light of this star from 3·6 to 4·2. It was therefore apparently observed by Herschel near its minimum. The light curve indicates that the observation preceded or followed a minimum by about nineteen hours, but the change in brightness during this time is much less than the uncertainty of the observation. The ephemeris of Schönfeld indicates a minimum for Ep. -2434 at 1795, Nov. 8^d $5^h{\cdot}6$, which does not differ from the time of observation by as much as the uncertainty of the comparison.

δ Cephei.—According to the curve of Argelander ‡, this star has the magnitudes of 3·5 and 4·3 at maximum and minimum. The observation of Herschel would correspond to 8·0 grades. This indicates a minimum preceding it by 1^d 2^h or 2^d 20^h, according as the light was increasing or decreasing. The star is above the horizon nearly all night, hence the time of observation is fixed

* Bonn Beob. vii. p. 329.
† Bonn Beob. vii. p. 389.
‡ Astron. Nach. xix. p. 395.

only by the limits of twilight. We may therefore call the time of observation, 1796, Nov. $5^d\ 12^h \pm 6^h$. The elements of Schönfeld give for Ep. −2987 a minimum at 1796, Nov. $4^d\ 7^h\ 24^m$, which agrees as well as could be desired with the observation. As in the case of η Aquilæ, the observations of contemporaneous observers fix the period of this star so accurately that a correction based upon a small number of observations does not seem justifiable.

β *Lyræ.*—The variations of this star have been so thoroughly determined by other observers that these observations cannot add much to our knowledge of the subject. Only one observation, No. 28 of Table I., is sufficiently precise to be of value, and the interval here employed —, is too large to be estimated with accuracy. This observation has therefore not been reduced.

R *Lyræ.*—The variations of this star are so small that it is doubtful if the observations of Herschel can be utilized.

ρ *Persei.*—The same remark may be applied to this star as to g Herculis.

λ *Tauri.*—This star belongs to the Algol class. The maximum brightness as given in the photometric catalogue is 3·6. The agreement of the observation No. 34 is probably accidental, since the large interval —, cannot be estimated with accuracy. As far as it goes, however, it indicates that the star was at its full brightness. The other observation, No. 33, indicates a diminution of light, or that the star was near a minimum. The law of variation of light is not known, but probably the change in magnitude amounts to about 0·8. The star retains its full brightness except for about two hours before and after each minimum. We may accordingly assume that a minimum preceded or followed the observation No. 33 by about one hour, On this day the sun set at about $3^h\ 47^m$, and λ Tauri set at $16^h\ 10^m$. Allowing for twilight, we may accordingly assume 1796, Jan. $1^d\ 10^h \pm 5^h$ for the time of observation. For the other date we obtain, in like manner, 1796, Nov. $30^d\ 11^h \pm 6^h$. The Ephemeris of Schönfeld, applying the equation of light, gives 1795, Dec. 31 $22^h{\cdot}6$, for Ep. −6500. A correction to the ephemeris of $-11^h{\cdot}5$ is thus indicated. This exceeds the possible error in the time, added to the probable error in magnitude. In other words, if the star was really below its full brightness, the minimum must have occurred several hours after the computed time. In like manner, we obtain 1796, Dec. $1^d\ 22^h{\cdot}3$ for Ep. −6485, or the nearest minimum does not occur until 35 hours after the observation No. 34. Accordingly, as the observation indicated, the star should have had its full brightness. The first minimum previously known of this star occurred on Dec. 6, 1848. If it were possible to determine the hour of Herschel's observation, the mean period of this star would be determined with great precision. An uncertainty of one hour would correspond to about half a second in a single period.

X *Sagittarii.*—This star varies in light from about 4·5 to 5·3 in a period of a little over seven days. The only comparison made by Herschel places this star a little fainter than 2 Sagittarii. The latter star is commonly placed in Ophiuchus; in fact it is nearly in line with 52 and 58 Ophiuchi, and between them. Its magnitude, according to the 'Uranometria Argentina,' is 6·8, which corresponds to 6·6 on the photometric scale. This would make the variable much too faint, even if at its minimum. It is also strange that Herschel should have employed a star at so great a distance (about 8°), when he might have taken others about equally faint and nearer. The hypothesis that 2 Sagittarii was much brighter then than now is negatived by the fact that Herschel compared it with 52 Ophiuchi, and found it only slightly brighter. The magnitude of this star in the 'Uranometria Argentina' is 6·5, corresponding to a photometric magnitude of 6·3. This value, although reducing the discrepancy, would still make the variable 6·5, which is 0·7 fainter than its light at minimum. This comparison is not given in the catalogue of Herschel, and accordingly is not checked by appearing under both 2 and 3 Sagittarii.

The southern declination of the star restricts the time of observation within narrow limits. The star sets at 9^h, and twilight would not be over until about 7^h. Accordingly, the time of observation would be 1795, Sept. $15^d\ 8^h \pm 1^h$. The elements of Schönfeld * give a minimum at 1795, Sept. $15^d\ 16^h$, for Ep. -3902. The period of Schmidt †, on the other hand, gives 1795, Sept. $13^d\ 22^h$. If, then, the star was really at its minimum when observed by Herschel, this observation determines a correction to the period with great certainty. EDWARD C. PICKERING.

[To be continued.]

Meteors of August 18–25, 1884.

ONE hundred and sixty-two meteors were recorded here on the nights of Aug. 18, 19, 20, 21, 22, 23, and 25, during watches reaching an aggregate of 18¼ hours. A very hazy sky on several of these dates prevented many being seen, hence the number observed is small relatively to the time employed in watching.

On August 19–20 some bright meteors with thick-spark trains and very slow motions radiated from $313° + 10°$ near Delphinus. I saw several of them falling in the northern sky. On the same dates I noticed some similar meteors from a centre in high N. declination at $75° + 78°$. The swift streak-leaving meteors were directed from $46° + 44°$ and $25° + 42°$.

On Aug. 21–25 11 were registered from a position very exactly defined at $70° + 50°$. The slow meteors, however, predominated, especially from the points $330° + 68°$, $331° + 7°$, $5° + 10°$, and $263° + 69°$, which supplied about 10 each. On these nights I also observed showers of short, quick meteors from $331° + 37$, $352° + 13°$, and $5° + 35°$. The latter were devoid of either streaks or trains.

* Zweiter Catalog. † Astron. Nach. lxxxvii. 109.

On August 23 two fine meteors, about equal to Jupiter, were seen; they left very bright streaks; the paths intersected at $41^\circ +15^\circ$.

The best observation of the series was on August 25, when I recorded 46 meteors between $12^h\ 15^m$ and 16^h. Two showers of swift meteors were singularly well defined that night from $30^\circ +36\frac{1}{2}^\circ$ and $62^\circ +37^\circ$. I have frequently determined these positions before in August, September, October, and November, and it is marvellous how the same identical radiant-points become manifested again and again to the almost absolute exclusion of adjacent spaces.

One noteworthy outcome of my late observations is that no paths whatever were recorded from the radiant at $291^\circ +60^\circ$, which furnished a very rich display of 52 meteors on August 21–23, 1879 ('Observatory,' Oct. 1879). Though I especially awaited the return of this conspicuous shower, it gave no sign, and its utter exhaustion for the present must be admitted. It is evidently a periodical display of some importance, and the determination of its cycle will become an essential feature of renewed observation. The companion shower near β Cephei, which I noticed in 1879, returned as usual this year, and I had seen it also on August 27, 1880, in each case from nearly the same point. The radiant is sharply defined and is apparently visible annually without much variation; it furnishes many brilliant slow-moving meteors, with their usual accompaniment of spark-trains.

Bristol, 1884, Sept. 3. W. F. Denning.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Eclipse of Thales.

Sir,—

As you have done me the honour of publishing several letters of mine on the subject of ancient eclipses, I should be glad if you could spare me space for another on that which it has been usual to call the Eclipse of Thales. *In limine*, I am aware that doubts have been expressed whether this was an eclipse at all; whilst the notion that it was predicted by Thales must be at once summarily rejected. With regard to the latter point, the only possible means available for forecasting eclipses in those days was by application of the period of 6585 days, which is known in modern times as the *saros* of the Chaldæans, though it seems to be doubtful whether we are right in understanding by that term the period in question. The Chaldæans, however, were certainly acquainted with this period of eclipses, by whatever name they called it, and Thales may have derived it from them. But although lunar eclipses may be predicted by its means, it would of course be quite impossible to form in the same way any idea in what part of the world an eclipse of the Sun would be visible, so that the so-called *saros* could not have been used for that purpose. And even if any one chose to make the very unlikely supposition that Thales was in possession of any better means of predicting a solar eclipse, he would assuredly in that case not have contented himself with merely announcing the *year* in which it might be expected. It has been well remarked therefore that this so-called prediction must rank with that of a good olive-crop by the same philosopher, and with other conjectures of a similar kind.

With regard to whether the phenomenon referred to by the historian was really an eclipse at all, perhaps I may be allowed to quote the passage from Herodotus (i. 74) in the original. After the war between the Lydians and Medians had continued for several years with varying success, it was renewed by a contest in the sixth year, when *συνήνεικε ὥστε τῆς μάχης συνεστεώσης τὴν ἡμέρην ἐξαπίνης νύκτα γενέσθαι.* Now on reading this, one's first impression, as Prof. Newcomb remarks, is that it is very doubtful whether an eclipse is alluded to, or whether it is simply a case of an overcast sky making the darkness of night appear to come on more quickly than usual. Nevertheless a consideration of all the circumstances does seem to make it probable that an eclipse of the Sun is meant, thereby enabling us (if this be accepted) to fix the exact date of the battle.

Much doubt has been entertained on this point; but the recent investigations of Prof. Newcomb seem fully to confirm the view expressed by Sir George Airy in his paper on ancient eclipses in the 'Philosophical Transactions' for 1853, that the event occurred on the 28th of May in the year B.C. 585 (chronological date, −584) when a large eclipse of the Sun was visible in Asia Minor late in the evening. Prof. Newcomb refers to the subject in an article "On the Recurrence of Solar Eclipses," printed as one of the "Astronomical Papers prepared for the use of the American Ephemeris and Nautical Almanac," which were published in one volume at Washington in 1882. After applying corrections determined by himself to Hansen's mean longitude of the Moon, he says (p. 55) "If these corrections are well founded, the Sun set upon the combatants about nine-tenths eclipsed." Now this appears to agree so well with the circumstance narrated by the historian of the sudden coming on of night (at a time when night was really drawing near), that I think it will be allowed to confirm in a remarkable manner that the eclipse of May 28th, B.C. 585, and the battle between the Lydian and Median armies took place at the same time. I may be allowed here to point out a singular mistake fallen into by Prof. Curtius in his Grecian History (book ii. ch. 5). He gives the correct date of the eclipse ("welche am 28sten Mai 585 v. Chr. im Halyslande den anbrechenden Tag in Nacht verwandelt hat"), but states that it occurred in the early morning instead of the late evening. Of course Prof. Newcomb's investigations had not been published when he wrote; but reference to Sir George Airy's paper would have shown him that the eclipse took place in the evening.

It is so interesting to have an early historic date so exactly fixed, that I crave room for a few words more on the historical significance of the matter. The King of Lydia at the time of the battle was certainly Alyattês, the father of Crœsus who succeeded him, probably in B.C. 559. The narrative of Herodotus appears to assume that Kyaxarês, the conqueror of Nineveh, was still reigning over the Medes. But if we accept B.C. 585 as the date of the battle we must suppose that the King of Media was Astyagês, who, so far as can be made out, succeeded his father Kyaxarês in B.C. 595. On this Mr. Grote remarks (History of Greece, 4th Edition, Vol. iii. p. 59, note) that Cicero (De Divinatione, i. 49) states that the eclipse predicted by Thales took place during the reign of Astyagês, whilst Pliny (Historia Naturalis, lib. ii. c. 12) assigns as the date of that eclipse the fourth year of the 48th Olympiad, which corresponds to B.C. 585. Upon the whole, then, it seems to me that we may now consider the date of this battle to be satisfactorily settled. Yours faithfully,

Blackheath, 1884, Aug. 30. W. T. Lynn.

The Red and White Spots on Jupiter.

Sir,—

On the morning of the 20th September, I obtained my first favourable sight of that portion of Jupiter which should be occupied by the red spot, and was much pleased to find that it still remains a visible object, although, in the present unfavourable position of the planet, one of extreme difficulty and delicacy. Definition was pretty fair, and with a power of 150 on my $5\frac{1}{4}$-inch Calver reflector, the spot was watched for about half an hour preceding, and for the same time following, the time of transit over the central meridian. So faint and difficult has the red spot become, that only a very occasional glimpse could be obtained of it at all, as a faint patch of no particular colour or boundary, until after the transit, when once, and once only, the spot was seen in its entirety, and then there seemed to be a distinct reddish tinge about it. It was estimated to be central at the following time, but of course without much accuracy :—

1884, September 19, $16^h\ 49^m$.

The great hollow in the red south equatoreal belt still remains visible, but it appears to have much diminished in plainness. The belt to the east and west of it is still distinctly and pretty easily seen to be double ; and this double character has a tendency to encroach upon the hollow, so as to reduce its depth and the concavity of the curve.

On the 18th September, I saw two of the equatoreal white spots on the central meridian, as follows :—

1884, September 18, $17^h\ 23^m$.
" " 18, 17 43

The latter of these is probably identical with the well-known white equatoreal spot, which has been followed for so many years; as, according to the ephemeris of Mr. Denning (Astron. Reg., August 1884, p. 185), that object should have been central at $17^h 45^m$ on Sept. 18th. Both these spots were rather bright, though, from the unfavourable circumstances attending their observations, difficult to be seen, and the times given are consequently rough. A third similar white spot was seen about 18° following the second of the above-mentioned objects. Yours faithfully,

West Brighton, A. STANLEY WILLIAMS.
1884, Sept. 22.

Comet 1884 b (*Barnard*).

SIR,—

A Kiel telegram announcing the discovery of this comet was received here from the Melbourne Observatory on July 23rd. It gave the position for July 16th, and stated that the daily motion was slow. A brief interval of clear sky on the evening of the 24th allowed of a search, when I found a very faint diffused nebulous object on the same parallel as the notified comet, but about three degrees more east than the Kiel position. It occurred in the same sweep as the bright nebula No. 3611 of Sir J. Herschel's Cape Results. I succeeded in obtaining four comparisons, which did not, however, indicate any decided proper motion in the object. Its position was found to be about a degree south of the faint nebula No. 3620 of Sir J. Herschel's Catalogue; but I did not notice this nebula in the sweep. A break in the clouds on the 26th enabled me to see that the suspicious object of the 24th was really a comet. I have observed the comet as often as I possibly could, and now avail myself of the mail just going out to send you such of my differential measures as I have already reduced. The measures have been made with the square bar-micrometer, and have been carefully corrected for defective orientation. I may state that star No. 5 crossed the intersection of the north bars, and did not therefore afford a satisfactory determination of the difference of north polar distance. I send the mean places of the stars of comparison with the authorities whence derived, but have not time to work out the reductions to the apparent places. As the comet cannot be seen at the northern European observatories, and imperfectly at the southern stations, the measures which I send may be of special interest. On the 28th the comet presented an indistinct condensation, which admitted of better observation. Since that date, however, it has been hardly visible, in consequence of the increasing moonlight. I have no doubt that when the Moon has withdrawn, it will admit of much more accurate observation.

In conclusion I may state that a very satisfactory series of observations of the Pons-Brooks comet, extending from January 13th to April 1st, has been recently forwarded to Europe.

I am, Sir, yours faithfully,

Windsor, N. S. Wales, JOHN TEBBUTT.
1884, Aug. 6th.

Observations of the Comet.

Windsor Mean Time 1884.	Comet—Star.		No. of Comps.	Comp. Star.
	Diff. App. R.A.	Diff. App. N.P.D.		
d h m s	m s	′ ″		
July 24 9 38 34	+ 7 39·54	−17 59·2	4	1
„ 24 9 38 34	+ 7 35·16	−15 21·0	4	2
„ 27 14 24 36	+ 2 2·99	−12 29·0	3	3
„ 27 14 24 36	− 4 42·53	− 2 22·5	3	4
„ 28 9 13 27	− 2 45·09	− 2 4·9	10	4
„ 28 10 40 7	+ 4 9·02	−12 25·2	5	3
„ 28 10 40 7	− 2 36·08	− 2 14·0	5	4
„ 31 9 41 34	+12 3·41	−13 26·0	7	3
„ 31 9 41 34	+ 5 18·21	− 3 15·7	7	4
„ 31 9 41 34	+ 6 40·35	......	7	5
„ 31 9 41 34	+ 2 23·29	+ 1 41·3	7	6

Mean Places of the Comparison Stars for 1884·0.

Comp. Star.	R.A.	N.P.D.	Authorities.
	h m s	° ″ ′	
1.	15 56 57·72	127 32 16·4	Wash. Mural Cir. *b*, 24·16; Wash. Cat. 1860, 6623; Cape Cat. 1880, 8727.
2.	15 57 2·14	127 29 40·9	Wash. Mural Cir. *b*, 24·17; Wash. Cat. 1860, 6624; Cape Cat. 1880, 8730.
3.	16 10 2·58	127 27 45·1	Wash. Mural Cir. *b*, 24·18.
4.	16 16 47·65	127 17 36·8	Wash. Mural Cir. *b*, 24·19; Wash. Cat. 1860, 6768; Cape Cat. 1880, 8913.
5.	16 15 24·88	127 8 52·4	Cape Cat. 1880, 8897.
6.	16 19 42·39	127 12 39·2	Anonymous. Equatoreal Comparisons.

Recent Measures of α Centauri.

SIR,—

A considerable period has, I believe, elapsed since any measures of this interesting star were published, and I therefore think that the results of observations taken by me during the present month will be acceptable. The position-angle and distance have both undergone a marked change since the last observation employed in Mr. Downing's paper in the 'R. A. S. Notices' for March last. My recent observations were taken both east and west of the meridian and within two hours of the upper culmination. The following are the results, the dates of observation being the 9th, 12th, and 18th :—

Epoch.	Eastern Measures.		Western Measures.		No. of Measures.
	Position-angle.	Distance.	Position-angle.	Distance.	
	°	"	°	"	
1884·522	199·5	12·92	200·9	13·01	10
·530	198·9	12·68	199·4	13·07	6
·546	198·9	12·98	201·2	12·92	8

On each evening the number of measures on each side of the meridian was the same, and there is obviously a tendency to make the position-angle greater on the western side. The results are perhaps as accordant as can be expected from so small an instrument as that employed, namely, an equatoreal of $4\frac{1}{2}$ inches aperture and 70 inches focal length. The power used was about 200 throughout, and the adopted value of one revolution of the micrometer-screw was $29''\cdot701$. Each individual result for distance was derived from a double measure in the usual way, and the steadiness and definition were moderately good.

Windsor, N.S. Wales, 1884, July 26.

I am, Sir, yours faithfully,
JOHN TEBBUTT.

The Distribution of the Stars.

SIR,—

On reading Professor Holden's paper on this subject in the last number of the 'Observatory,' it occurred to me that better results might possibly be obtained by taking together each pair of sectors separated by 12 hours in R.A. I have accordingly compiled the following table by simply adding and dividing Professor Holden's figures. In the first column, I. signifies the stars whose R.A. ranges from 0^h to 1^h and also those from 12^h to 13^h; II. those ranging from 1^h to 2^h and also from 13^h to 14^h, and so on.

In the second column I give the average number of stars to each degree from Argelander's map; in the third column the average number of stars to each degree from Herschel's gauges. Palisa's and Peters's maps I could not use, for one of the two sectors which I desired to take together was usually wanting either wholly or in part; but Peters gives for X., XI., and XII. of the first column 75·0, 67·6, and 58·7 respectively—figures agreeing well with Argelander, all of them, moreover, being under the "weighted mean" of Peters, which is 80·1. I give the means for Argelander and Herschel at the foot of the second and third columns for reference. It will be seen that, with one doubtful exception (X.), the figures in the second and third columns always depart from the mean in the same direction, the departure being, however, much wider in the case of the smaller stars.

I remain, truly yours,

2 Bath St., Portrush, 1884, Sept. 15. W. H. S. Monck.

Table.

Sectors examined.	Stars to each degree. Argelander.	Herschel.
I.	12·2	263
II.	12·1	228
III.	11·9	240
IV.	11·7	225
V.	13·1	294
VI.	18·0	791
VII.	22·0	1541
VIII.	21·2	2863
IX.	18·6	1313
X.	15·1	803
XI.	13·7	262
XII.	12·2	167
Means	15·15	749·2

Fine Display of Aurora.

Sir,—

Perhaps you may be interested in hearing that I had the pleasure, on the evening of the 20th of August, of seeing a beautiful display of Aurora from this ship during our voyage to Montreal.

After five days of rough weather, with strong westerly winds, the sky cleared at sunset for the first time, and when the stars appeared, bright cloud-like wreaths swept across the heavens from W. to E., rapidly shifting their position and varying in form every moment. There was no doubt as to their nature, for very soon the distinctive perpendicular streamers shot from them towards the zenith, sometimes in long columns, sometimes in soft fringes of light. There was none of the peculiar rose colour, pale yellow predominating.

This wonderful display went on increasing in beauty, at one time culminating in the north, though no arch of light was visible, till the eye was tired of watching it, and the icy wind blowing from the Greenland coast warned us to retire; but it certainly lasted for several hours, and the man on watch told me that he saw it again at 3 o'clock in the morning, and from his account it is not uncommon in these latitudes.

At the time of its occurrence we must have been nearly in Lat. 53° 30′, Long. 50° W. Believe me,

Yours sincerely,
E. Brown.

S.S. 'Polynesian,'
1884, Aug. 22.

The Eclipse of Pericles, and the Eclipses of A.D. 1191 *and* A.D. 1733.

Sir,—

(1) Eclipse of Pericles. In addition to the difficulty involved in supposing an eclipse of about three-fourths of the Sun's disk to cause some of the stars to shine out as Thucydides records, it appears there are historical difficulties which render it probable that we must look for some other eclipse than that of B.C. 431 as the eclipse of Pericles. Parker in his 'Chronology' (1858), p. 783, admits that the eclipses of B.C. 431, 424, 413, agree with the account which Thucydides has given of his three eclipses, but adds "they place the end of the Peloponnesian war only at the distance of 81 years above the death of Alexander. We need not again go over our manifold testimonies to show that this is *utterly incredible.*"

(2) Eclipse of 1191. In an article headed 'Historical Sun-darkenings' by Dr. Hind, in 'Nature' of June 26, 1879, we have the following:—"A century later, in June 1191, according to Schnurrer, the Sun was again darkened with certain attendant effects upon nature: here the cause is easily found; on June 23 there was a total eclipse in which the Moon's shadow traversed the continent of Europe from Holland to the Crimea; the eclipse was total in this country between the coasts of Cumberland and Yorkshire." Instead of being total, I find the Moon's semidiameter was very small at the time, so that the eclipse could only have been widely annular.

(3) Eclipse of 1733. This was the last of a nine-year series of four fine eclipses in our land, of which the two middle ones, 1715 and 1724, were total in England. Weaver's Ephemeris for 1733 makes the line of totality in May 1733 to run across the northern isles of Shetland. My own rough calculations make the northernmost of the Shetland Islands (Unst) to lie a good way south of the total phase, according to which the inhabitants would have to make a very considerable excursion out to sea to get involved in total darkness. But if any record should exist which would lead to the supposition that totality touched the northern point of these islands, it would be extremely interesting to bring it forward, as in this case the eclipse of 1733 would be the last total eclipse of the Sun visible in Great Britain; and the next nearest approach to totality in our kingdom is that of 1954, only just escaping the northernmost of the Shetlands (if we omit that of 1927, total for a very few seconds).

The above eclipse of 1733 was the last one in Europe that was total for a period of 109 years, no other taking place till 1842—something like the interval between two transits of Venus. *Query*: When did a whole century go by before without one total solar eclipse in Europe? Vassenius, at Gottenburgh, says:—

"7.14.6 adparebat ♃.

"7.14.46 incepit totus tegi solis discus.

"7.15.50 maximæ tenebræ, cum stellæ omnes Ursæ Majoris, cor ♌, Procyon, Sirius, oculus ♉, et nonnullæ aliæ videri poterant." It lasted $2^m\ 8^s$. He speaks of "subrubicundæ nonnulæ maculæ extra peripheriam disci lunaris."

Faithfully yours,

S. J. Johnson.

Melplash Vicarage, Bridport,
Sept. 19.

The Effect of Moonlight and thin Cloud on the Visibility of Meteors.

Sir,—

I think Mr. Corder, in his letter on the last shower of Perseids in the 'Observatory' for September, has not made sufficient allowance for the hindrance to visibility of meteors by moonlight &c. Not only would the fainter meteors be hidden, but the brighter ones diminished in number. I do not know to what extent this would take place, or of any calculations definitely determining it; but there is no doubt that moonlight would prevent bright meteors from being seen at so great a distance from the point of sight as on a clear moonless night. The brighter the meteor the further it can be seen, though its character has doubtless an effect on the distance of visibility; *e. g.*, meteors with long paths would be seen at a greater distance from the point of sight than those with short paths.

In the following Table I have assumed the average distances at which meteors of various magnitudes can be seen:—

Meteors of	Assumed radius of field of visibility.
	°
7 mag.	5, if any.
6 "	10
5 "	25
4 "	35
3 "	45
2 "	55
1 "	65
= Sirius	75
= Jupiter	85
= Venus	100
= Venus at its brightest	113
= 5 times Venus	180

If the distance of visibility in a horizontal direction be that given in the above table, then the distance a meteor can be seen above or below the point of sight will not be so great. By making the limit of visibility of the brightest meteors to extend to 180°, it is meant that the light would be visible in any part of the sky, though if the meteor appeared behind the observer, he might not be able to turn round fast enough to see it.

If we accept this table as approximately true, it appears that moonlight and light cloud sufficient to reduce a 1st magnitude meteor to apparent 3rd magnitude would reduce the distance it could be seen from the point of sight from 65° to 45°, and the total number of such meteors visible would be reduced by about 40 per cent.

Yours truly,

Sunderland, 1884, Sept. 19 T. W. Backhouse.

NOTES.

First Principles of Natural Philosophy *.—The first edition of this little work was published several years ago, and was intended chiefly for students preparing to pass the matriculation examination of the London University, or other Examinations in which the elements of Natural Philosophy are included as a subject. It consisted of five sections, the first two being devoted to Mechanics divided into Statics and Dynamics, the third to Hydrostatics and Hydrodynamics, the fourth to Pneumatics, and the fifth to Optics, understanding by the latter Geometrical Optics only. In the second edition, now published, a sixth section has been added, on the nature of Light and Sound, which will, we think, increase the utility of the work.

The Photometry of Saturn's Ring †.—In this paper, Prof. Seeliger enters into some investigations with the view of pointing out the knowledge that he thinks may be obtained with regard to the constitution of Saturn's ring by means of photometrical observations of the amount of light reflected from it at different times. Had it been a body of continuous surface, the apparent intensity of its illumination would (unless, indeed, assumptions were made of an altogether improbable kind as to its structure) be very different in different relative positions of the Sun as well as the Earth. Changes of this nature are not, as a matter of fact, indicated by observation, the apparent brightness of the ring being always nearly the same, and the amount of light received from it would seem to depend entirely upon the proportion of the whole surface which is turned towards the Earth, or upon the angle of elevation of the Earth above the plane of the ring. Hence Zöllner concluded that Lambert's law of photometry was not applicable in this case. But Prof. Seeliger shows that, under certain plausible assumptions, the observed effects are consistent with that law, the

* 'The First Principles of Natural Philosophy.' Second Edition. By William Thynne Lynn, B.A., F.R.A.S., formerly of the Royal Observatory, Greenwich. (Van Voorst.)

† Astron. Nachr. No. 2612.

extent of application of which can hardly, he thinks, be over-estimated. Maxwell pointed out, 25 years ago, from purely mechanical considerations, that the ring could not be a compact solid or fluid mass, but must consist of a number of separate discrete particles similar to those which compose a meteoric stream. On this supposition, the observed photometric conditions admit of a simple explanation, though their full significance cannot be worked out until more accurate observations have been made with regard to the variations in intensity of the light of the ring at different times. Prof. Seeliger's hope is that the investigations and considerations brought forward by him, in the paper before us, may have the effect of interesting photometric observers in the subject, and inducing them to devote more attention to it than has yet been done.

The Lunar Eclipse of October 4.—The duration of the total phase in the approaching eclipse is somewhat longer than usual; and though the Moon will then be traversing a region barren of bright stars, Prof. Struvé has drawn up a list of 116 small stars which will suffer occultation during the eclipse. Some sixty observatories will, it is expected, cooperate in the work of observation, and it is anticipated that "a far more exact value of the Moon's true diameter" may be secured in the single night of Oct. 4 "than can be obtained by long-continued ordinary observations of occultations, where, on the same night, only one of the two distinct phenomena (disappearance or reappearance) can be observed on the dark limb."

The following are the times of disappearance and reappearance as furnished by Prof. Struvé for all the stars down to the 10th mag. which will be occulted by the Moon at Greenwich during the total eclipse. The angles are counted from the true North point in the usual direction.

Disappearances.

Star's No.	Mag.	Angle. °	G.M.T. h	m	s
62	10–11	100·3	8	24	54
61	9·4	12·0		44	42
69	9·5	109·7		50	28
74	9·3	109·5	9	0	33
63	8·7	4·7		1	55
71*	10	156·0		13	
76	10	144·6		15	36
85	10	82·5		25	4
82	10	112·2		25	37
81	9·5	42·8		33	51
94	9–10	39·2	10	7	3
95	9·5	46·1		12	40
106	10	43·8		37	19
108	9–10	78·7		37	45
109	8·8	79·6		37	48
107	10	69.7		38	57
96	10	7·7		39	12

Reappearances.

Star's No.	Mag.	Angle. °	G.M.T. h	m	s
62	10–11	214·2	9	20	30
61	9·4	302·3		22	52
76	10	169·1		30	0
63	8·7	308·8		33	43
69	9·5	203·9		39	53
74	9·3	204·0		50	35
82	10	201·6	10	14	53
85	10	230·1		32	7
81	9·5	269·7		38	14
94	9–10	272·8	11	11	1
96	10	304·2		16	59
95	9·5	265·9		20	4
106	10	268·1		44	16

* A near approach. The star will be 5″·4 S. of the limb.

Diameter and Figure of Uranus.—Some important contributions have recently been made to our knowledge of the diameter of Uranus, amongst which may be particularized the researches of Seeliger, Millosevich, and Schiaparelli. Prof. Seeliger *, observing with the $10\frac{1}{2}''$ refractor and Repsold's micrometer of the Munich Observatory, has used a reflecting rectangular prism placed between the eyepiece and the eye, and capable of rotation round the optical axis of the telescope, so that the micrometer-threads may appear to be placed in any direction that may be desired. With this apparatus Prof. Seeliger has made a series of measures of the diameter of Uranus in four different directions, in order, if possible, to detect any ellipticity in the disk that may exist, some measures being made with the micrometer-threads vertical and some with the threads horizontal. Prof. Seeliger, however, arrives at the conclusion that no sensible deviation from circularity in the disk of the planet can be discovered from his measures. For although certain differences exist when the measures in four different directions made on different nights are taken together, these disappear when measures made on the same night are combined, and may be traced, in Prof. Seeliger's opinion, to differences in the atmospheric conditions under which the observations were taken. There also appear to be differences in the mean diameter, measured without the prism, with the prism placed so that the threads appear vertical, and with it placed so that the threads appear horizontal; but these can hardly be considered to be well established, as the observations are not very numerous. On the whole Prof. Seeliger thinks that the most probable value of the mean diameter of Uranus at mean distance (19·1826), derivable from his observations, is $3''\cdot915 \pm 0''\cdot045$, the combined result from former determinations being $3''\cdot823$. Observations of the diameter have also been made and recently published by Prof. Millosevich †, using the 9″ Merz equatoreal of the observatory of the Collegio Romano and a filar micrometer, generally with illuminated field and dark threads. The Roman astronomer finds that in position-angle 0°, the diameter of Uranus at the distance the log. of which is 1·245 is $3''\cdot963$, and in position-angle 90° the diameter is $3''\cdot982$, or practically identical with the former; there is therefore no evidence of ellipticity, and the combined mean diameter is $3''\cdot9648$. On the other hand, Prof. Schiaparelli ‡, of Milan, concludes from his observations that there is a measurable ellipticity in the disk of Uranus. In the series of observations undertaken by him in the early part of the present year, in continuation of, and generally on the same plan as, those made in 1883 ('Observatory,' vol. vi. p. 275), Prof. Schiaparelli proposed to

* Sitzungsbericht der math.-phys. Classe der k. bayer. Akad. d. Wiss. 1884, Heft 2.

† Reale Accademia dei Lincei, Serie 3, Vol. xix.—Seduta del 2 dicembre, 1883.

‡ Astronomische Nachrichten, No. 2608.

take measures of the (supposed) equatoreal and polar diameters of the planet, with the precaution of observing sometimes with the line joining the eyes parallel and sometimes perpendicular to the direction of the equatoreal diameter, so as to get rid of any possible systematic error arising from observing in one or other of these directions. Bad weather, however, prevented him from carrying out this plan, as when he was able to commence observations the planet was in such a position that it was only possible to observe with the line joining the eyes perpendicular to the equatoreal diameter. Prof. Schiaparelli's measures result in an ellipticity $\frac{1}{13{\cdot}04 \pm 1{\cdot}01}$, a result agreeing within the limits of probable error with that which he obtained in 1883, viz. $\frac{1}{10{\cdot}94 \pm 0{\cdot}67}$. A few observations in this series were made without placing the line joining the eyes either parallel or perpendicular to the equatoreal diameter of the planet, the reduction of which gives for the ellipticity of the disk $\frac{1}{11{\cdot}44 \pm 1{\cdot}92}$, agreeing well with the result obtained from all the measures, and showing, Prof. Schiaparelli thinks, that his observations are not affected by systematic error depending on direction of measurement.

Two distinguished astronomers, Mädler and Schiaparelli, have deduced from their measures of the disk of Uranus values of the ellipticity which agree very closely, as also to a less extent do their determinations of the position-angle of the greater axis. But Mädler's measures of distances of double stars are affected by large systematic errors depending on the direction of the measured distance with respect to the horizontal line; and therefore but little reliance can be placed on his results.

It is to be hoped that Prof. Schiaparelli will continue his researches on the figure of Uranus, taking every precaution that is feasible in order to avoid the possibility of systematic error affecting the result. A. M. W. D.

The Red Spot on Jupiter.—This object was reobserved by Mr. Denning on the morning of Sept. 22, with a power of 252 on a 10-inch reflector. The spot was barely discernible as a faint greyish patch, merging from a dark belt on its southern side, with which it is now connected along its whole length. Definition was good when the planet was first seen at $17^h\ 45^m$, but the markings soon grew feeble and indefinite as the Sun rose. The depression in the great S. belt still appears very conspicuous, and its longitude coincides, as formerly, with the position of the relic of the red spot. The two objects were as nearly as possible central at

September 21, $18^h\ 28^m$;

but the Sun was shining so brightly at the time, that Mr. Denning regards the observation as probably not very accurate. The depression in the belt was, however, very well seen, and offers an excellent guide for determinations of position.

Mr. Stone's Explanation of the Errors of Hansen's Lunar Tables *.—The Editors of the 'Bulletin Astronomique,' in their abridgment of the report of the meeting of the Royal Astronomical Society of June 13, given in No. 87 of the 'Observatory,' have added the following remarks:—"M. Stone has addressed some remarks to the 'Bulletin' with regard to the reservations which accompanied the analysis of his communications to the Royal Astronomical Society, and it is right to add some brief explanations to what has been already said. M. Stone admits, as the basis of his theory, that the ratio $\frac{n}{\omega}$ of the mean motion of the Sun in longitude to the movement of the equinoctial point with regard to the meridian ought not to change when we pass from Bessel's Tables to those of Le Verrier, and his formula for the change of the unit of time T,

$$\frac{\delta T}{T}=\frac{\delta n}{n},$$

supposes the condition referred to. But, as it has been explained (Bulletin, tome i. p. 92), the ratio $\frac{n}{\omega}$ is a fact of observation, for the observations of the Sun being reduced and referred to mean time through *the medium of the equation of time*, the resolution of the equations of condition will furnish in particular the mean sidereal motion of the Sun in a mean day, that is to say, in arc, the ratio $\frac{n}{\omega-n}$; it does not, then, seem permissible to suppose the ratio $\frac{n}{\omega}$ constant; and if this condition is introduced into the formulæ, it is necessary to bear in mind that in process of time the observations will not be equally represented. As to the small variation of the mean solar day which could arise from a new discussion of the observations of the Sun, it is very minute, as several writers (MM. Gaillot, Adams, Cayley) have pointed out. Let us refer the two periods T and T′ to an intermediate interval of time, and let us suppose that n and ω are the arcs described in this interval of time, we shall have on the first theory

$$T(\omega-n)=2\pi;$$

and so, if we consider the second theory,

$$T'(\omega-n')=2\pi$$

(*because* $\omega'=\omega$, *the Earth turning uniformly on its axis*).

From which

$$\frac{T'}{T}=\frac{\omega-n}{\omega-n'};\quad \frac{T'-T}{T'}=\frac{n'-n}{\omega-n}."$$

Clocks of Precision.—We learn from 'Science' that the Seth Thomas Clock Company, aided by the advice of Dr. Waldo, has

* Bulletin Astronomique, 1884 Aug., p. 398.

undertaken the construction of clocks of a high degree of excellence for scientific purposes. Considerable progress has been made with experiments to ascertain the best form of pendulum-suspension and dimensions of the steel-jar mercurial pendulum (which is filled *in vacuo* by a new process). A small laborat ·y is being erected for the investigation of such questions as the permanency of lengths of pendulum-r ..s of different materials, the effect of air mechanically contained in the ordinary mercurial pendulums, the effect of mercuric oxide and other impurities of the mercury, and the effect of temperature-changes on various forms of pendulum-suspension. It is proposed to designate the clocks "clocks of precision."

PHOTOGRAPHY AS A MEANS OF CHARTING STARS *.—In a recent communication to the Paris Academy of Sciences Admiral Mouchez, Director of the Paris Observatory, states that MM. Henry, finding it almost impossible, on account of the great number of stars, to chart the part of the heavens which they have now reached by the methods hitherto adopted, have had recourse to photography. Their first attempt with a provisional apparatus has been so successful that Admiral Mouchez considers that the problem will soon be solved. Proofs of plates taken with a telescope $0^{m}\cdot16$ in diameter and $2^{m}\cdot10$ focal length, corrected for the photographic rays, were submitted to the Meeting. Each plate contains a part of the sky extending 2° in R.A. by 3° in Dec., and contains about 1500 stars from the 6th to 12th magnitude, *i. e.* to the limit of visibility of an instrument of the size used. The results have induced MM. Henry to undertake the construction of a powerful instrument for this class of work, and they are now engaged in constructing an object-glass of $0^{m}\cdot34$ diameter, which will be corrected for the photographic rays and so constructed as to cover clearly and without distortion the greatest possible space. In reference to the advantages of this method of charting, Admiral Mouchez points out that work which ordinarily takes several months to perform can be done in a single hour. It is considered that with the new apparatus stars to the 13th or even 14th magnitude will probably be secured.

OLBERS'S COMET OF 1815.—A sweeping ephemeris for this comet is given by Herr Ginzel of Vienna, for the period Oct. 1 to Dec. 31, in the 'Astronomische Nachrichten,' No. 2613–14. The computed date of perihelion passage is 1886 December 17; but the 1815 observations leave an uncertainty in the period of $\pm 1\cdot6$ year, so that it may be well that the search for it be commenced at once.

COMET 1884 *b* (BARNARD).—This comet was observed by Mr. Common at Ealing on Sept. 22 with his 3-foot reflector, in which it was well seen. On the following evening Mr. Prince saw it with his telescope by Tulley of 6·8 in. aperture. He describes it as "an exceedingly faint nebulous object without

* Comptes Rendus, Vol. xcix. No. 7.

central condensation or nucleus of any kind." No telescope of smaller aperture than five inches would, he thought, suffice to show it. Mr. Common assigns it a diameter of at least 4'.

The following ephemeris is extracted from one given in the 'Astronomische Nachrichten,' No. 2615:—

Berlin. Midnight.	R.A. h m s	Dec. ° '	Log r.	Log Δ.
Oct. 1....	20 20 14	−23 58·1	0·1419	9·8132
5....	20 33 51	−22 30·1	0·1474	9·8331
9....	20 46 54	−21 2·5	0·1532	9·8536
13....	20 59 26	−19 35·6	0·1592	9·8746
17....	21 11 28	−18 10·0	0·1654	9·8960
21....	21 23 4	−16 45·9	0·1718	9·9175
25....	21 34 14	−15 23·5	0·1783	9·9391
29....	21 45 1	−14 2·9	0·1850	9·9608

Yale College Observatory.—Dr. Elkin, the partner of Dr. Gill in the heliometer work at the Cape, took charge of the large heliometer of the Yale College Observatory on Jan. 15 last, the expenses attending the use of the instrument for three years being guaranteed by ten subscribers. In addition to the necessary scale determinations, &c., Dr. Elkin secured about one third of the number of measures proposed for a triangulation of the Pleiades to be compared with the measures of Bessel and Wolf. Measures of the diameter of Venus near inferior conjunction, and determinations of places on the Moon relative to stars, with a view to determining the parallactic inequality in the Moon's motion, have also been secured.

The Heliometer for the Cape Observatory.—The Treasury have sanctioned the £2700 required for the 7-inch Heliometer requisitioned by Dr. Gill for the Cape Observatory. A contract has been entered into with Messrs. Repsold of Hamburg, who expect to complete the instrument about Dec. 1886.

Comet Pons-Brooks was seen till June 2 by Mr. A. S. Atkinson, of Nelson, N.Z.

A new Minor Planet, No. 240, of the twelfth magnitude, was discovered on August 27, by M. Borrelly, at Marseille. Another, No. 241, of the eleventh magnitude, was discovered on Sept. 12, by Dr. Luther, at Dusseldorf. A third, of the 13th magnitude, No. 242, was discovered by Herr Palisa at Vienna, on Sept. 22.

We regret to notice the death, on Sept. 7, of Mr. J. Birmingham, Millbrook, Tuam, Ireland. Mr. Birmingham was well known as the discoverer of the variable star T Coronæ, and as the author of a Catalogue of Red Stars.

Dr. Heinrich Schellen, author of the well-known and valuable work on Spectrum Analysis, died at Cologne on Sept. 5, aged sixty-six years.

Maxima and Minima of Variable Stars in 1884, *October.*

M, signifies maximum ; *m*, minimum.

Oct.	2·5	ζ Geminorum, M.
	3·6	W Virginis, *m*.
	5	U Monocerotis, *m*.
	7	R Lyræ, M.
	7·6	ζ Geminorum, *m*.
	9	S Delphini, *m*.
	10	T Ursæ Maj., M.
	11·4	β Lyræ, *m*.
	11·6	T Monocerotis, *m*.
	11·8	W Virginis, M.
	12·6	ζ Geminorum, M.
	14	R Cassiopeiæ, M.
	15	V Cancri, M.
	15	S Vulpeculæ, M.
	16	R Carini, *m*.
	17·8	ζ Geminorum, *m*.
Oct.	19	T Serpentis, M.
	19·5	T Monocerotis, M.
	20·9	W Virginis, *m*.
	21	S Arietis, M.
	22	V Tauri, M.
	22·8	ζ Geminorum, M.
	24	*o* Ceti, *m*.
	24·3	β Lyræ, *m*.
	25	U Cygni, *m*.
	27·9	ζ Geminorum, *m*.
	28	R Crateris, *m*.
	29	U Monocerotis, M.
	29·1	W Virginis, M.
	30	L^2 Puppis, M.
	31	R Coronæ, M.
	31	S Scorpii, M.

Variables of Short Period.

δ Cephei.

	d.
Oct.	1·65, *m*.
	3·25, M.
	7·05, *m*.
	8·65, M.
	12·35, *m*.
	17·75, *m*.
	19·35, M.
	24·75, M.
	28·45, *m*.

W Sagittarii.

	d.
Oct.	10·5, *m*
	13·6, *m*.
	21·2, M.
	25·7, *m*.
	28·8, M.

η Aquilæ.

	d.
Oct.	9·4, M.
	16·6, M.
	21·4, *m*.
	23·7, M.
	28·5, *m*.

U Sagittarii.

	d.
Oct.	2·3, M.
	12·8, *m*.
	15·8, M.
	19·5, *m*.
	22·5, M.
	26·3, *m*.
	29·2, M.

X Sagittarii.

	d.
Oct.	2·5, M.
	6·6, *m*.
	9·5, M.
	13·6, *m*.
	16·5, M.
	20·7, *m*.
	23·5, M.
	27·7, *m*.
	30·5, M.

Algol, *m*.

		h	m
Oct.	3	5	32
	14	16	48
	17	13	37
	20	10	26
	23	7	15

U Cephei, *m*.

		h	m
Oct.	3	8	44
	8	8	24
	13	8	3
	18	7	43
	23	7	22
	28	7	2
	30	18	51

λ Tauri, *m*.

		h	m
Oct.	4	8	53
	8	7	45
	12	6	38
	24	18	13
	28	17	5

S Cancri, *m*.

		h	m
Oct.	1	9	56
	20	9	11

δ Libræ, *m*.

		h	m
Oct.	1	10	49
	3	18	40
	8	10	23

U Coronæ, *m*.

		h	m
Oct.	3	12	6
	10	9	49
	17	7	31
	20	18	22
	27	16	5

Elements and Ephemeris of Comet 1884 *c* (*Wolf*)*.

Dr. Wolf, of Heidelberg, discovered a new comet on Sept. 17. From observations of it made on Sept. 21, 22, and 23, Mr. S. C. Chandler, Jr., has computed the following elements:—

T = 1884 November 26·03 G.M.T.

$\pi - \Omega$	180° 59′	Mean Equinox 1884·0.
Ω	199 31	
i	31 22	
log q	0·22016	

Ephemeris for Greenwich Midnight.

1884.	R.A. h m s	Dec. ° ′	Brightness.
Sept. 24..........	21 17 24	+20 34	1·11
28..........	21 19 52	18 42	
Oct. 2..........	21 23 16	16 43	
6..........	21 27 20	+14 40	1·27

The comet was observed by Lt.-Col. Tupman as follows:—

Sept. 24 10^h 38^m, R.A. 21^h 27^m 23^s, Dec. +20° 36′ 16″.

He describes it as showing a stellar nucleus, 3″ in diam. The coma was faint and diffused, and 2′ in. diam. It was a rather bright telescopic comet, and might just be seen with a binocular.

Differences of Right Ascension and Declination between Iapetus and the Centre of Saturn. By A. Marth †.

Iapetus − Saturn.

Greenwich Noon.	R.A. s	Dec. ″	Greenwich Noon.	R.A. s	Dec. ″
Oct. 1....	−38·3	−118	Oct. 17....	− 7·8	−164
2....	−38·1	−128	18....	− 4·6	−157
3....	−37·6	−138	19....	− 1·4	−149
4....	−36·8	−147	20....	+ 1·9	−140
5....	−35·8	−155	21....	+ 5·1	−131
6....	−34·6	−162	22....	+ 8·4	−120
7....	−33·1	−168	23....	+11·5	−109
8....	−31·4	−173	24....	+14·7	− 96
9....	−29·4	−177	25....	+17·7	− 84
10....	−27·3	−179	26....	+20·7	− 70
11....	−24·9	−181	27....	+23·5	− 57
12....	−22·4	−181	28....	+26·2	− 42
13....	−19·7	−180	29....	+28·7	− 28
14....	−16·9	−177	30....	+31·1	− 13
15....	−14·0	−174	31....	+33·3	+ 2
16....	−10·9	−169			

* Dun-Echt Circular, No. 90.
† Monthly Notices, Vol. xliv. No. 8.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

Mimas (East).

		h
Oct.	1	14·0
	2	12·6
	15	17·2
	16	15·8
	17	14·4
	18	13·0
	19	11·6
	20	10·2
	21	8·8
	31	17·7

Mimas (West).

		h
Oct.	12	10·0
	13	8·6
	23	17·4
	24	16·0
	25	14·6
	26	13·3
	27	11·7
	28	10·3
	29	8·9

Hyperion.

		h
Sept.	26	16·6
Oct.	17	20·3

Enceladus.

		h
Oct.	1	11·5
	2	20·4
	4	5·2
	5	14·1
	6	23·0
	8	7·9
	9	16·8
	11	1·6
	12	10·5
	13	19·4
	15	4·3
	16	13·2
	17	22·1
	19	7·0
	20	15·9
	22	0·7
	23	9·6
	24	18·4
	26	3·3
	27	12·2
	28	21·1
	30	6·0
	31	14·9

Tethys.

		h
Oct.	1	23·1
	3	20·5
	5	17·8
	7	15·1
	9	12·4
	11	9·6
	13	6·9
	15	4·2
	17	1·5
	18	22·9
	20	20·2
	22	17·5
	24	14·8
	26	12·0
	28	9·3
	30	6·6

Titan.

		h
Sept.	30	15·0
Oct.	16	13·7

Dione.

		h
Oct.	3	1·6
	5	19·2
	8	12·9
	11	6·6
	14	0·3
	16	17·9
	19	11·6
	22	5·3
	24	23·0
	27	16·6
	30	10·3

Rhea.

		h
Oct.	5	10·2
	9	22·7
	14	11·0
	18	23·4
	23	11·8
	28	0·2

The following table will furnish the means for inferring roughly the distances and position-angles of the satellites from the diagram given on page 272.

		Position-angle of Minor Axis of Ring.
Sept.	30	354° 15′
Oct.	10	354 15
	20	354 16
	30	354 18

		Semi-major axis of orbit of Titan.
Sept.	30	192″·8
Oct.	10	196 ·3
	20	199 ·7
	30	202 ·8

Astronomical Memoranda, 1884, *October.*

Equation of Time:—Sun *before* Clock, Oct. 1, $10^m\ 31^s$; Oct. 11, $13^m\ 22^s$; Oct. 21, $15^m\ 23^s$; Oct. 31, $16^m\ 17^s$.

Sidereal Time at Mean Noon:—Oct. 1, $12^h\ 42^m\ 29^s$; Oct. 11, $13^h\ 21^m\ 55^s$; Oct. 21, $14^h\ 1^m\ 20^s$; Oct. 31, $14^h\ 40^m\ 46^s$.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Oct. 3....	18 8	5 31	26 22	+6 29	83 31
8....	18 17	5 19	26 30	6 10	17 34
13....	18 25	5 8	26 27	5 49	311 36
18....	18 33	4 58	26 12	5 25	245 40
23....	18 42	4 48	25 46	4 59	179 43
28....	18 51	4 38	25 9	+4 30	113 46

The position-angles of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon.

Moon.	*sets.* h m		*rises.* h m		*sets.* h m
Oct. 1..	14 54	Oct. 11..	10 50	Oct. 21..	6 20
2..	16 6	12..	12 1	22..	6 58
3..	17 21	13..	13 11	23..	7 42
4..	*rises*	14..	14 21	24..	8 31
5..	5 50	15..	15 30	25..	9 25
6..	6 24	16..	16 39	26..	10 24
7..	7 3	17..	17 46	27..	11 27
8..	7 49	18..	*sets*	28..	12 33
9..	8 43	19..	5 17	29..	13 42
10..	9 44	20..	5 47	30..	14 54
				31..	16 9

Full Moon, Oct. 4, $10^h\ 0^m$; Last Quarter, Oct. 11, $2^h\ 29^m$; New Moon, Oct. 18, $12^h\ 31^m$; First Quarter, Oct. 26, $16^h\ 54^m$.

Mercury, a morning star, at greatest elongation (17° 55′ W.) Oct. $4^d\ 15^h$.

Venus, a morning star in Leo, in conjunction with the Moon Oct. $14^d\ 20^h$. Diameter:—Oct. 1, 21″·0; Oct. 31, 16″·6. Illuminated portion of disk 0·622 on Oct. 15.

	R.A.	Dec.	tr.	rises
Oct. 1,	$9^h\ 40^m$·0,	13° 3′ N.,	$20^h\ 58^m$,	$13^h\ 48^m$
31,	11 48 ·3,	2 42 N.,	21 8	14 53

Mars in Libra, in conjunction with the Moon Oct. $20^d\ 23^h$. Diameter:—Oct. 1, 4″·9; Oct. 31, 4″·8. Illuminated portion of disk 0·971 on Oct. 15.

	R.A.	Dec.	tr.	sets
Oct. 1,	$14^h\ 40^m$·7,	15°·59′ S.,	$1^h\ 58^m$,	$6^h\ 38^m$
31,	16 7 ·5,	21 35 S.,	1 26	5 33

Jupiter near Regulus, in conjunction with the Moon Oct. $14^d\ 5^h$. Diameter:—Oct. 1, 30″·4; Oct. 31, 32″·6.

	R.A.	Dec.	tr.	rises
Oct. 1,	$9^h\ 58^m$·1,	13° 13′ N.,	$21^h\ 13^m$,	$14^h\ 0^m$
31,	10 17 ·3,	11 33 N.,	19 34	12 32

Saturn, stationary Oct. $5^d\ 15^h$, in conjunction with the Moon Oct. $9^d\ 9^h$.

	R.A.	Dec.	tr.	rises
Oct. 1,	$5^h\ 35^m$·1,	21° 52′ N.,	$16^h\ 50^m$,	$8^h\ 44^m$
31,	5 32 ·7,	21 48 N.,	14 49	6 45

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
Oct. 20......	44″·38	19″·81	29″·51	13″·18	17″·6

The south side of the rings is visible, the elevation of the Earth above their plane on Oct. 20 being 26° 31′ S., and of the Sun 26° 47′ S.

Uranus.

	R.A.	Dec.	tr.	rises
Oct. 1,	$11^h\ 58^m{\cdot}2$,	0° 58′ N.,	$23^h\ 12^m$,	$17^h\ 3^m$
31,	12 4 ·7,	0 16 N.,	21 21	15 16

Neptune.

	R.A.	Dec.	tr.	rises
Oct. 1,	$3^h\ 24^m{\cdot}3$,	16° 48′ N.,	$14^h\ 39^m$,	$7^h\ 6^m$
31,	3 21 ·5,	16 37 N.,	12 39	5 7

Total eclipse of the Moon Oct. 4, visible at Greenwich.

First contact with penumbra Oct. 4, $7^h\ 16^m{\cdot}8$, with shadow $8^h\ 15^m{\cdot}2$; beginning of total phase $9^h\ 15^m{\cdot}8$; middle of eclipse $10^h\ 2^m{\cdot}0$; end of total phase $10^h\ 48^m{\cdot}2$; last contact with shadow $11^h\ 48^m{\cdot}8$, with penumbra $12^h\ 47^m{\cdot}2$. Magnitude of the eclipse (Moon's diameter = 1) 1·525.

The first contact with shadow takes place at 83 degrees to the E. of N. point of Moon's limb; and last contact at 118 degrees towards the W., for *direct* image.

A partial eclipse of the Sun Oct. 18, invisible at Greenwich. Begins on the Earth generally Oct. 18, $10^h\ 19^m{\cdot}9$, in Long. 131° 56′ E., Lat. 63° 28′ N. Greatest eclipse Oct. 18, $12^h\ 17^m{\cdot}9$, in Long. 130° 3′ W., Lat. 71° 33′ N., nearly two-thirds of the Sun's diameter being obscured. Ends on the Earth generally Oct. 18, $14^h\ 15^m{\cdot}8$, in Long. 134° 21′ W., Lat. 33° 20′ N.

Phenomena.

	G. M. T. h	m	
Oct. 1	12	40	θ Aquarii Oc. D. 48°.
	13	8	θ Aquarii Oc. R. 357°.
	15	31	J. i. Tr. I.
2	15	12	J. i. Oc. R.
3	8	12	B.A.C. 8311 Oc. D. 97°.
5	16	0	J. ii. Ec. D.
6	10	2	38 Arietis Oc. R. 319°.
7	15	54	J. ii. Tr. E.
8	17	29	J. i. Tr. I.
9	10	37	130 Tauri Oc. R. 259°.
	17	10	J. i. Oc. R.
	18	13	B.A.C. 1930 Oc. R. 293°.
10	15	34	J. iii. Oc. R.
12	12	58	A² Cancri Oc. R. 320°.
	17	29	60 Cancri Oc. R. 326°.
Oct. 12	18	1	α Cancri Oc. D. 107°.
	19	4	α Cancri Oc. R. 219°.
14	15	42	J. ii. Tr. I.
16	15	48	J. i. Ec. D.
17	15	34	J. iii. Ec. R.
	16	9	J. iii. Oc. D.
	16	17	J. i. Tr. E.
18	13	38	J. i. Oc. R.
23	15	36	J. ii. Oc. R.
	17	42	J. i. Ec. D.
24	15	55	J. i. Tr. I.
	15	57	J. iii. Ec. D.
25	15	35	J. i. Oc. R.
27	8	44	8 Aquarii Oc. D. 87°.
28	14	3	J. iii. Tr. E.
30	7	26	11 Piscium Oc. D. 134°.
	10	27	14 Piscium Oc. D. 133°.
31	17	52	J. i. Tr. I.

The angles are reckoned from the *apparent* N. point towards the right of the Moon's inverted image.

EDITOR.

ERRATUM IN No. 88.

Page 231, second line from foot, *for* 1846 *read* 1826.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 91. NOVEMBER 1. 1884.

Meeting of the Liverpool Astronomical Society.

The Fourth Session was opened on October 13, at the Association Hall, the President, the Rev. T. E. Espin, B.A., F.R.A.S., occupying the chair. Messrs. R. S. Newall, F.R.S., F.R.A.S., E. Crossley, F.R.A.S., M. Aronsberg, Samuel Grove, T. M. Frames, and Miss M. Ashley were elected Members of the Society.

A paper on the adjustment of equatoreal telescopes, by Mr. Howard Grubb, F.R.S., F.R.A.S., formed the opening subject of the Session. In forwarding this communication Mr. Grubb wished it to be understood that no novelty was claimed for any of the methods; but as the directions given in different treatises on astronomy differed more or less from each other, the amateur was often puzzled as to the best method to adopt. He therefore submitted to the Society the methods which he had himself found best under varying circumstances and conditions. [The paper will be printed *in extenso* in a future number of the 'Observatory.']

In reviewing the past Session, the President attributed a great portion of the success of the Society to the hand of good fellowship which was held out to all lovers of the science of Astronomy, whether they were beginners or advanced students. He was very proud to say that, within the last year, two very important publications on stellar photometry had been given to the world by Associates of the Society. The first, by Professor Pritchard, was published in Vol. xlvii. of the 'Memoirs' of the R.A.S. The second, by Professor E. C. Pickering, was received from Harvard only a few days ago. Professor Pritchard's magnitudes were determined by a wedge of neutral tinted glass, and those of Prof. Pickering by an ingenious meridian photometer, in which the Pole star and the star to be observed were placed side by side. The scales of both were assumed from the Pole star, but there was a difference of one-tenth of a magnitude in their assumed magnitudes of the standard of comparison, the Harvard observations giving it as 2·15 and the Oxford as 2·05.

The following results were obtained from a comparison of 456 stars, amongst which no double stars and suspected variables were included, and show the number of stars and the percentage of the total number compared for every tenth of a magnitude of difference between the two authorities.

Difference in magnitude.	Number of stars.	Percentage.
0·0 – 0·1	146	32·0
0·1 – 0·2	125	27·4
0·2 – 0·3	66	14·4
0·3 – 0·4	59	12·9
0·4 – 0·5	27	5·9
0·5 – 0·6	16	3·5
0·6 – 0·7	8	1·7
0·7 – 0·8	4	0·8
0·8 – 0·9	2	0·4
0·9 – 1·0	3	0·6

From a comparison of 39 constellations the table seemed to show (1) that the Oxford magnitudes were lower than the Harvard, and (2) that the minus correction predominated in the Galactic constellations. Of 34 stars that differed more than half a magnitude, 13 were white, 17 yellow, 3 orange, and 1 green. It seemed therefore that the differences were not altogether due to colour.

Mr. Thomas Gwyn Elger, F.R.A.S., stated that in closely examining the neighbourhood of the great Alpine valley, with his $8\frac{1}{2}$-in. Calver under a power of 400, he had seen a peculiar formation which the Rev. Preb. Webb had aptly compared to a Florence oil-flask. A little to the north he had also noted a bright projection from the mountain mass, on the eastern side of the valley, which appeared to almost entirely close the passage. At times of best definition, however, a dark space was just visible between the extreme end of this promontory and the west side of the valley. The shadow of this object showed that it was a spur or buttress running athwart the gorge near its narrowest part. On the S.W. side of the oval expansion there was also a row of small white objects resembling craterlets. The whole of this region he had carefully delineated, as he thought the great Alpine valley would form a very suitable object of study for the Lunar section of the Society. It had been urged that observers should confine their attention to one formation; but it seemed to him that much valuable time and many favourable opportunities might be lost by strictly adhering to this plan, as several lunations might pass by without a glimpse of the selected object.

In a paper on the Triesnecker and Hyginus clefts, M. Gaudibert called attention to the difficulties of observation of this part of the lunar surface. There were three classes of objects which had chiefly occupied observers—the clefts, the craterlets, and the supposed new objects known as Klein's Craters. The clefts, notwithstanding all the efforts which had been made, remained still most mysterious objects; and it seemed that, as their number and extension increased, ignorance as to their mode of formation and the purpose they fulfilled increased in proportion. If we only knew the valley which emerges from the north side of Herodotus, or the wedge-shaped valley of the Alps, we might perhaps rest satisfied with the theory of Gruithuisen, that they were old dried river-beds, though the last named would have been a wide river indeed. But, even if we could admit of such an interpretation in the systems of clefts to be met with elsewhere, the difficulties here would still be insuperable.

On the subject of suspected variables in Andromeda, Mr. W. S. Franks, F.R.A.S., pointed out what he considered an error in Mr. Gage's paper ('Abstracts,' Vol. ii. p. 71), where it is stated that o and π Andromeda and B.A.C. 8245 are not variable, and should be eliminated from the list of suspects, because they were sensibly constant for a period of four months. Mr. Franks thought it was quite possible for those stars to remain constant for that period, and, if they belonged to Class III. of Prof. Pickering's types, be still variable, although they might escape detection in so short a time. He was inclined to think it would be better to observe these stars, say once a month, during the time they were favourably situated, for several consecutive years, before arriving at so confident a conclusion as to their non-variability.

Mr. W. H. Gage, F.R.A.S., submitted the first instalment of a list of coloured stars. He had taken up this subject because it had been pointed out by Professor Smyth that this branch of investigation might lead to interesting results. His plan had been to observe stars not less than 10° north of the equator, and with an aperture of not less than $4\frac{1}{4}$ inches. Great care had been taken in the estimations, and the colours had been limited to various shades of orange, red, and yellow. No star less than the seventh magnitude had been observed for colour. He had found, with one exception, that of 15 constellations there was a tendency to one colour in each. For example, in Gemini there were 12 yellow, 6 orange, 6 white, and 3 red. In Cygnus he had observed 18 yellow, 2 white, and no orange or red. He had also noticed a tendency for stars near one another to agree in colour.

The Rev. E. B. Kirk related some interesting particulars of veiled spots, mentioned by Trouvelot and the Rev. S. J. Perry, which he had observed with his $6\frac{1}{2}$-inch reflector by Browning. The peculiarity was the rapid disappearance of the spots and also their rapid change of shape. He had found the veiled spots very difficult objects, owing to their nearly uniformly bright surface, which caused the eye to wander to some more definite points, such as large spots or the edge of the disk.

Mr. Joseph Baxendell, F.R.S., F.R.A.S., said he had observed the late eclipse from the Birkdale Observatory, under moderately favourable circumstances. Some of the occulted stars could not be seen with sufficient distinctness to determine the exact times of their disappearance or reappearance, but he had noticed that both were instantaneous. He had timed the disappearance of a 9·5 magnitude star, not in Struve's list, at $23^h\ 26^m\ 33^s{\cdot}18$, the angle of disappearance being about 130°.

Mr. Baxendell also submitted the results of 25 years' observation of the variable star T Herculis, from the data of which he had obtained a maximum period of 87·42 days and a minimum period of 164·47 days, the increase of brightness being therefore more rapid than the decrease. The extreme difference between the calculated and observed times of maxima was 11·57 days, and the mean difference 4·20 days. The corresponding differences of the minima were 10·07 and 4·10 days respectively. The value of the mean period, derived by Professor Schönfeld from observations extending over 35 periods, was 165·1 days. The ruddy colour of this variable was much more decided at the minimum, and, indeed, in some maxima it almost entirely disappeared. Its magnitude at maximum varied from 7·1 to 8·0, and at minimum from 12·6 to 13·5. The calculated date of the next maximum would be Nov. 8, 1884.

Pending Problems of Astronomy.

[Continued from p. 283.]

In the planetary system we meet, in the main, the same problems as those that relate to the Moon, with a few cases of special interest.

For the most part, the accordance between theory and observation in the motions of the larger planets is as close as could be expected. The labours of Leverrier, Hill, Newcomb, and others have so nearly cleared the field, that it seems likely that several decades will be needed to develop discrepancies sufficient to furnish any important corrections to our present tables. Leverrier himself, however, indicated one striking and significant exception to the general tractableness of the planets. Mercury, the nearest to the Sun, and the one, therefore, which ought to be the best behaved of all, is rebellious to a certain extent: the perihelion of its orbit moves around the Sun more rapidly than can be explained by the action of the other known planets. The evidence to this effect has been continually accumulating ever since Leverrier first announced the fact, some thirty years ago; and the recent investigation by Professor Newcomb of the whole series of observed transits puts the thing beyond question. Leverrier's own belief (in which he died) was, that the effect is due to an unknown planet or planets between Mercury and the Sun; but, as things now stand, we think that any candid investigator must admit that the probability of the existence of any such body or bodies of considerable dimensions is vanishingly small. We do not forget the numerous instances of round spots seen on the solar disk, nor the eclipse-stars of Watson, Swift, Trouvelot, and others; but the demonstrated possibility of error or mistake in all these cases, and the tremendous array of negative evidence from the most trustworthy observers, with the best equipment and opportunity, makes it little short of certain that there is no Vulcan in the planetary system.

A ring of meteoric matter between the planet and the Sun might account for the motion of the perihelion; but, as Newcomb has suggested, such a ring would also disturb the *nodes* of Mercury's orbit.

It has been surmised that the cause may be something in the distribution of matter within the solar globe, or some variation in gravitation from the exact law of the inverse square, or some supplementary electric or magnetic action of the Sun, or some special effect of the solar radiation, sensible on account of the planet's proximity, or something peculiar to the region in which the planet moves; but as yet no satisfactory explanation has been established.

Speaking of unknown planets, we are rather reluctantly obliged to admit that it is a part of our scientific duty as astronomers to continue to search for the remaining asteroids; at least, I suppose so, although the family has already become embarrassingly large. Still I think we are likely to learn as much about the constitution, genesis, and history of the solar system from these little flying rocks as from their larger relatives; and the theory of perturbations will be forced to rapid growth in dealing with the effects of Jupiter and Saturn upon their motions.

Nor is it unlikely that some day the searcher for these insignificant little vagabonds may be rewarded by the discovery of some great world, as yet unknown, slow moving in the outer desolation beyond the remotest of the present planetary family. Some configurations in certain cometary orbits, and some almost evanescent peculiarities in Neptune's motions, have been thought to point to the existence of such a world; and there is no evidence, nor even a presumption, against it.

Mercury as yet defies all our attempts to ascertain the length of its day, and the character and condition of its surface. Apparently the instruments and methods now at command are insufficient to cope with the difficulties of the problem; and it is not easy to say how it can be successfully attacked.

With Venus, the Earth's twin-sister, the state of things is a little better: we do already know, with some degree of approximation, her period of rotation; and the observations of the last few months bid fair, if followed up, to determine the position of her poles, and possibly to give us some knowledge of her mountains, continents, and seas.

It would be rash to say of Mars that we have reached the limit of possible knowledge as regards a planet's surface; but the main facts are now determined, and we have a rather surprising amount of supposed knowledge regarding his geography. By "supposed" I mean merely to insinuate a modest doubt whether some of the map-makers have not gone into a little more elaborate detail than the circumstances warrant. At any rate, while the "areographies" agree very well with each other in respect to the planet's more important features, they differ widely and irreconcilably in minor points.

As regards the physical features of the asteroids, we at present know practically nothing; the field is absolutely open. Whether it is worth any thing may be a question; and yet, if one *could* reach it, I am persuaded that a knowledge of the substance, form, density, rotation, temperature, and other physical characteristics, of one of these little orphans, would throw vivid light on the nature and behaviour of interplanetary space, and would be of great use in establishing the physical theory of the solar system.

The planet Jupiter, lordliest of them all, still, as from the first, presents problems of the highest importance and interest. A sort of connecting-link between suns and planets, it seems as if, perhaps, we might find, in the beautiful and varied phenomena he exhibits, a kind of halfway house between familiar terrestrial facts and solar mysteries. It seems quite certain that no analogies drawn from the Earth and the Earth's atmosphere alone will explain the strange things seen upon his disk, some of which, especially the anomalous differences observed between the rotation periods derived from the observation of markings in different latitudes, are very similar to what we find upon the Sun. "The great red spot" which has just disappeared, after challenging for several years our best endeavours to understand and explain it, still, I think, remains as much a mystery as ever,—a mystery probably hiding within itself the master-key to the constitution of the great orb of whose inmost nature it was an outward and most characteristic expression. The same characteristics are also probably manifested in other less conspicuous but equally curious and interesting markings on the varied and ever-changing countenance of this planet; so that, like the Moon, it will well repay the most minute and assiduous study.

Its satellite system also deserves careful observation, especially in respect to the eclipses which occur; since we find in them a measure of the time required for light to cross the orbit of the Earth, and so of the solar parallax, and also because, as has been already mentioned, they furnish a test of the constancy of the Earth's rotation. The photometric method of observing these eclipses, first instituted by Professor Pickering at Cambridge in 1878, and since re-invented by Cornu in Paris, has already much increased the precision of the results.

With reference to the mathematical theory of the motion of these satellites, the same remarks apply as to the planetary theory. As yet nothing appears in the problem to be beyond the power and scope of existing methods, when carried out with the necessary care and prolixity; but a new and more compendious method is most desirable.

The problems of Saturn are much the same as those of Jupiter, excepting that the surface and atmospheric phenomena are less striking, and more difficult of observation. But we have, in addition, the wonderful rings, unique in the heavens, the loveliest of all telescopic objects, the type and pattern, I suppose, of world-making, in actual progress before our eyes. There seems to be continually accumulating evidence from the observations of Struve, Dawes, Henry, and others, that these whirling clouds are changing in their dimensions and in the density of their different parts; and it is certainly the duty of every one who has a good telescope, a sharp eye, and a chastened imagination, to watch them carefully, and set down exactly what he sees. It may well be that even a few decades will develop most important and instructive phenomena in this gauzy girdle of old Chronos. Great care, however, is needed in order not to mistake fancies and illusions for solid facts. Not a few anomalous appearances have been described and commented on, which failed to be recognized by more cautious observers with less vivid imaginations, more trustworthy eyes, and better telescopes.

The outer planets, Uranus and Neptune, until recently, have defied all attempts to study their surface and physical characteristics. Their own motions and those of their satellites have been well worked out; but it remains to discuss their rotation, topography, and atmospheric peculiarities. So remote are they, and so faintly illuminated, that the task seems almost hopeless; and yet, within the last year or two, some of our great telescopes have revealed faint and evanescent markings upon Uranus, which may in time lead to some further knowledge of that far-off relative. It may, perhaps, be that some great telescope of the future will give us some such views of Neptune as we now get of Jupiter.

There is a special reason for attempts to determine the rotation-periods of the planets, in the fact that there is very possibly some connection between these periods, on the one hand, and, on the other, the planets' distances from the Sun, their diameters and masses. More than thirty years ago, Professor Kirkwood supposed that he had discovered the relation in the analogy which bears his name. The materials for testing and establishing it were then, however, insufficient, and still remain so, leaving far too many of the data uncertain and arbitrary. Could such a relation be discovered, it could hardly fail to have a most important significance with respect to theories of the origin and development of the planetary system.

The great problem of the absolute dimensions of our system is, of course, commanded by the special problem of the solar parallax; and this remains a problem still. Constant errors of one kind or another, the origin of which is still obscure, seem to affect the different methods of solution. Thus, while experiments upon the velocity of light and heliometric measurements of the displacements of Mars among the stars agree remarkably in assigning a smaller parallax (and greater distance of the Sun) than seems to be indicated by the observations of the late transits of Venus, and by methods founded on the lunar motions, on the other hand, the meridian observations of Mars all point to a larger parallax and smaller distance. While still disposed to put more confidence in the methods first named, I, for one, must admit that the margin of probable error seems to me to have been rather increased than diminished by the latest published results deduced from the transits. I do not feel so confident of the correctness of the value 8″·80 for the solar parallax as I did three years ago. In its very nature, this problem is one, however, that astronomers can never have done with. So fundamental is it, that the time will never come when they can properly give up the attempt to increase the precision of their determination, and to test the received value by every new method that may be found.

The problems presented by the Sun alone might themselves well occupy more than the time at our disposal this evening. Its mass, dimensions, and motions, as a whole, are, indeed, pretty well determined and understood; but when we come to questions relating to its constitution, the cause and nature of the appearances presented upon its surface, the periodicity of its spots, its temperature, and the maintenance of its heat, the extent of its atmosphere, and the nature of the corona, we find the most radical differences of opinion.

The difficulties of all solar problems are, of course, greatly enhanced by the enormous difference between solar conditions and the conditions attainable in our laboratories. We often reach, indeed, similarity sufficient to establish a bond of connection, and to afford a basis for speculation; but the dissimilarity remains so great as to render quantitative calculations unsafe, and make positive conclusions more or less insecure. We can pretty confidently infer the presence of iron and hydrogen and other elements in the Sun by appearances which we can reproduce upon the Earth; but we cannot safely apply empirical formulæ (like that of Dulong and Petit, for instance), deduced from terrestrial experiments, to determine solar temperatures: such a proceeding is an unsound and unwarrantable extrapolation, likely to lead to widely erroneous conclusions.

For my own part, I feel satisfied as to the substantial correctness of the generally received theory of the Sun's constitution, which regards this body as a great ball of intensely heated vapours and gases, clothed outwardly with a coat of dazzling clouds formed by the condensation of the less volatile substances into drops and crystals like rain and snow. Yet it must be acknowledged that this hypothesis is called in question by high authorities, who maintain, with Kirchhoff and Zöllner, that the visible photosphere is no mere layer of clouds, but either a solid crust, or a liquid ocean of molten metals; and there may be some who continue to hold the view of the elder Herschel (still quoted as authoritative in numerous school-books), that the central core of the Sun is a solid and even habitable globe, having the outer surface of its atmosphere covered with a sheet of flame maintained by some action of the matter diffused in the space through which the system is rushing. We must admit that the question of the Sun's constitution is not yet beyond debate.

And not only the constitution of the Sun itself, but the nature and condition of the matter composing it, is open to question. Have we to do with iron and sodium and hydrogen as we know them on the Earth, or are the solar substances in some different and more elemental state?

However confident many of us may be as to the general theory of the constitution of the Sun, very few, I imagine, would maintain that the full explanation of sun-spots and their behaviour has yet been reached. We meet continually with phenomena, which, if not really contradictory to prevalent ideas, at least do not find in them an easy explanation.

So far as mere visual appearances are concerned, I think it must be conceded that the most natural conception is that of a dark chip or scale thrown up from beneath, like scum in a caldron, and floating, partly submerged, in the blazing flames of the photosphere which overhang its edges, and bridge across it, and cover it with filmy veils, until at last it settles down again and disappears. It hardly *looks* like a mere hollow filled with cooler vapour, nor is its appearance that of a cyclone seen from above. But, then, on the other hand, its spectrum under high dispersion is very peculiar; not at all that of a solid, heated slag, but it is made up of countless fine dark lines, packed almost in contact, showing, however, here and there, a bright line, or at least an interspace where the rank is broken by an interval wider than that which elsewhere separates the elementary lines,—a spectrum, which, so far as I know, has not yet found an analogue in any laboratory experiment. It seems, however, to belong to the type of absorption spectra, and to indicate, as the accepted theory requires, that the spot is dark in consequence of *loss* of light, and not from any original defect of luminosity. Here, certainly, are problems that require solution.

The problem of the Sun's peculiar rotation and equatorial acceleration is a most important one and still unsolved. Probably its solution depends in some way upon a correct understanding of the exchanges of matter going on between the interior and the surface of the fluid cooling globe. It is a significant fact (already alluded to), that a similar relation appears to hold upon the disk of Jupiter; the bright spots near the equator of the planet completing their rotation about five minutes more quickly than the great red spot, which was forty degrees from the equator. It is hardly necessary to say that an astronomer, watching our terrestrial clouds from some external station (on the Moon, for instance), would observe just the reverse. Equatorial clouds would complete their revolution more slowly than those in our own latitude. *Our* storms travel toward the east, while the volcanic dust from Krakatoa moved swiftly west. We may at least conjecture that the difference between different planets somehow turns upon the question, whether the body whose atmospheric currents we observe is receiving more heat from without than it is throwing off itself. Whatever may be the true explanation of this peculiarity in the motion of sun-spots, it will, when reached, probably carry with it the solution of many other mysteries, and will arbitrate conclusively between rival hypotheses.

The periodicity of the sun-spots suggests a number of important and interesting problems; relating, on the one hand, to its mysterious cause, and, on the other, to the possible effects of this periodicity upon the Earth and its inhabitants. I am no "sun-spottist" myself, and am more than sceptical whether the terrestrial influence of sun-spots amounts to any thing worth speaking of, except in the direction of magnetism. But all must concede that this is by no means yet demonstrated (it is not easy to prove a negative); and there certainly are facts and presumptions enough tending the other way to warrant more extended investigation of the subject. The investigation is embarrassed by the circumstance, pointed out by Dr. Gould, that the effects of sun-spot periodicity, if they exist at all (as he maintains they do), are likely to be quite different in different portions of the Earth. The influence of changes in the amount of the solar radiation will, he says, be first and chiefly felt in alterations and deflections of the prevailing winds, thus varying the *distribution* of heat and rain upon the surface of the Earth without necessarily much changing its absolute amount. In some regions it may, therefore, be warmer and dryer during a sun-spot maximum, while in adjoining countries it is the reverse.

There can be no question, that it is now one of the most important and pressing problems of observational astronomy to devise apparatus and methods delicate enough to enable the student to follow promptly and accurately the presumable changes in the daily, even the hourly, amounts of the solar radiation. It might, perhaps, be possible with existing instruments to obtain results of extreme value from observations kept up with persistence and scrupulous care for several years at the top of some rainless mountain, if such can be found; but the undertaking would be a difficult and serious affair, quite beyond any private means.

Related to this subject is the problem of the connection between the activity of the solar surface and magnetic disturbances on the Earth,—a connection unquestionable as matter of fact, but at present unexplained as matter of theory. It may have something to do with the remarkable prominence of iron in the list of solar materials; or the explanation may, perhaps, be found in the mechanism by means of which the radiations of light and heat traverse interplanetary space, presenting itself ultimately as a corollary of the perfected electro-magnetic theory of light.

The chromosphere and prominences present several problems of interest. One of the most fruitful of them relates to the spectroscopic phenomena at the base of the chromosphere, and especially to the strange differences in the behaviour of different spectrum-lines, which, according to terrestrial observations, are due to the same material. Of two lines (of iron, for instance) side by side in the spectrum, one will glow and blaze, while the other will sulk in imperturbable darkness; one will be distorted and shattered, presumably by the swift motion of the iron vapour to which it is due, while the other stands stiff and straight.

Evidently there is some deep-lying cause for such differences; and as yet no satisfactory explanation appears to me to have been reached, though much ingenious speculation has been expended upon it. Mr. Lockyer's bold and fertile hypothesis, already alluded to, that at solar and stellar temperatures our elements are decomposed into others more elemental yet, seems to have failed of demonstration thus far, and rather to have lost ground of late; and yet one is almost tempted to say, "It *ought* to be true," and to add that there is more than a possibility that its essential truth will be established some time in the future.

Probably all that can be safely said at present is, that the spectrum of a metallic vapour (iron, for instance, as before) depends not only upon the chemical element concerned, but also upon its physical conditions; so that, at different levels in the solar atmosphere, the spectrum of the iron will differ greatly as regards the relative conspicuousness of different lines; and so it will happen that, whenever any mass of iron vapour is suffering disturbance, those lines only which particularly characterize the spectrum of iron in that special state will be distorted or reversed, while all their sisters will remain serene.

The problem of the solar corona is at present receiving much attention. The most recent investigations respecting it—those of Dr. Huggins and Professor Hastings—tend in directions which appear to be diametrically opposite. Dr. Huggins considers that he has succeeded in photographing the corona in full sunshine, and so in establishing its objective reality as an immense solar appendage, sub-permanent in form, and rotating with the globe to which it is attached. One may call it "an atmosphere," if the word is not to be too rigidly interpreted. I am bound to say that plates which he has obtained do really show just such appearances as would be produced by such a solar appendage, though they are very faint and ghost-like. I may add further, that, from a letter from Dr. Huggins, recently received, I learn that he has been prevented from obtaining any similar plates in England this summer by the atmospheric haze, but that Dr. Woods, who has been provided with a similar apparatus, and sent to the Riffelberg in Switzerland, writes that he has "an assured success."

Our American astronomer, on the other hand, at the last eclipse (in the Pacific Ocean), observed certain phenomena which seem to confirm a theory he had formulated some time ago, and to indicate that the lovely apparition is an apparition only, a purely optical effect due to the *diffraction* (not *refraction*, nor reflection either) of light at the edge of the Moon—no more a solar appendage than a rainbow or a mock sun. There are mathematical considerations connected with the theory which may prove decisive when the paper of its ingenious and able proposer comes to be published in full. In the mean time it must be frankly conceded that the observations made by him are very awkward to explain on any other hypothesis.

Whatever may be the result, the investigation of the status and possible extent of a nebulous envelope around a sun or a star is unquestionably a problem of very great interest and importance. We shall be compelled, I believe, as in the case of comets, to recognize other forces than gravity, heat, and ordinary gaseous elasticity, as concerned in the phenomena. As regards the actual existence of an extensive gaseous envelope around the Sun, I may add that other appearances than those seen at an eclipse seem to demonstrate it beyond question,—phenomena such as the original formation of clouds of incandescent hydrogen at high elevations, and the forms and motions of the loftiest prominences.

But of all solar problems, the one which excites the deepest and most general interest is that relating to the solar heat, its maintenance and its duration. For my own part, I find no fault with the solution proposed by Helmholtz, who accounts for it mainly by the slow contraction of the solar sphere. The only objection of much force is, that it apparently limits the past duration of the solar system to a period not much exceeding some twenty millions of years; and many of our geological friends protest against so scanty an allowance. The same theory would give us, perhaps, half as much time for our remaining lifetime; but this is no objection, since I perceive no reason to doubt the final cessation of the Sun's activity, and the consequent death of the system. But while this hypothesis seems fairly to meet the requirements of the case, and to be a necessary consequence of the best knowledge we can obtain as to the genesis of our system and the constitution of the Sun itself, it must, of course, be conceded that it does not yet admit of any observational verification. No measurements within our power can test it, so far as we can see at present.

It may be admitted, too, that much can be said in favour of other theories; such as the one which attributes the solar heat to the impact of meteoric matter, and that other most interesting and ingenious theory of the late Sir William Siemens.

As regards the former, however, I see no escape from the conclusion, that, if it were exclusively true, the Earth ought to be receiving, as was pointed out by the late Professor Peirce, as much heat from meteors as from the Sun. This would require the fall of a quantity of meteoric matter,—more than sixty million times as much as the best estimates make our present supply, and such as could not escape the most casual observation, since it would amount to more than a hundred and fifty * tons a day on every square mile.

As regards the theory of Siemens, the matter has been, of late, so thoroughly discussed, that we probably need spend no time upon it here. To say nothing as to the difficulties connected with the establishment of such a far-reaching vortex as it demands, nor of the fact that the temperature of the Sun's surface appears to be above that of the dissociation point of carbon compounds, and hence above the highest heat of their combustion, it seems certainly demonstrated, that matter of the necessary density could not exist in interplanetary space without seriously affecting the planetary motions by its gravitating action as well as by its direct resistance; nor could the stellar radiations reach us, as they do, through a medium capable of taking up and utilizing the rays of the Sun in the way this theory supposes.

And yet I imagine that there is a very general sympathy with the feeling that led to the proposal of the theory,—an uncomfortable dissatisfaction with received theories, because they admit that the greater part of the Sun's radiant energy is, speaking from a scientific point of view, simply wasted. Nothing like a millionth part of the sky, as seen from the Sun, is occupied, so far as we can make out, by objects upon which its rays can fall: the rest is vacancy. If the Sun sends out rays in all directions alike, not one of them in a million finds a target, or accomplishes any useful work, unless there is in space some medium to utilize the rays, or unknown worlds of which we have no cognizance beyond the stars.

Now, for my own part, I am very little troubled by accusations of wastefulness against Nature, or by demands for theories which will show what the human mind can recognize as "use" for all energy expended. Where I can perceive such use, I recognize it with reverence and gratitude, I hope; but the failure to recognize it in other cases creates in my mind no presumption against the wisdom of Nature, or against the correctness of an hypothesis otherwise satisfactory. It merely suggests human limitations and ignorance. How can one without sight understand what a telescope is good for?

At the same time, perhaps, we assume with a little too much confidence, that, in free space, radiation does take place equally in

* In an article on astronomical collisions, published in the 'North-American Review' about a year ago, I wrongly stated the amount at fifty tons. There was some fatality connected with my calculations for that article. I gave the amount of heat due to the five hundred tons of meteoric matter which is supposed to fall daily upon the Earth with an average velocity of fifteen miles per second as fifty-three calories annually per square metre,—a quantity two thousand times too great. Probably the error would have been noticed if even the number given had not been so small, compared with the solar heat, as fully to justify my argument, which is only strengthened by the correction. I owe the correction to Professor Le Conte of California, who called my attention to the errors.

the difference amounts to four tenths of a magnitude. Applying this correction, the results become more readily comparable without affecting the conclusions derived from them. In Table V. the brightness of δ at the time of the various observations, and at maximum and minimum, as stated in the first column, is compared. The following columns give the magnitude derived in Table IV. from the three authorities, after applying a correction of four tenths of a magnitude to the 'Uranometria Argentina.' The last column gives a mean value which may be employed, since all the observations have been reduced to the same scale.

TABLE V.

	H. P.	W. H.	U. A.	Mean.
1795, May 11	5·2	5·1	5·3	5·2
1795, May 18	5·4	5·2	5·3	5·3
" "	5·6	5·2	5·5	5·4
1797, May 22	—	5·9	5·8	5·8
Maximum	4·9	—	5·1	5·0
Minimum	5·8	—	6·0	5·9

A minimum is clearly indicated in 1797, May 22, and the star seems to have been below its maximum brightness on the other nights also. Converting the magnitudes into grades, and comparing with the light curve of Professor Schönfeld, we may infer that on 1795, May 11, the observation was made $4^h·7$ before, or $5^h·2$ after, a minimum. The observations of 1795, May 18, in like manner, indicate that a minimum would follow in $3^h·4$, or had passed $3^h·8$. The observation of 1797, May 22, indicates a brightness that does not differ sensibly from that at minimum. The star changes in brightness by about a tenth of a magnitude within an hour of minimum.

The hours within which the observations must have been made are limited by the twilight, which would, for observations of such faint stars, be appreciable within two hours of midnight. The comparison stars 43 and 44 Libræ would be above the horizon from $8^h·0$ to $16^h·4$. Their altitudes would exceed 10° from $9^h·2$ to $15^h·2$.

The period of δ Libræ is $2^d\ 7^h\ 51^m\ 20^s = 2^d·3273148$. Accordingly, if a minimum occurred near midnight on 1797, May 22, others would have occurred on the afternoons of 1795, May 11, and 1795, May 18. We may therefore assume that the observations of 1795 were made after, and not before minima. Subtracting from the time 1795, May $11^d\ 12^h$, the interval $5^h·2$, adding $0^h·2$ for the difference in longitude of Slough and Paris, and adding $0^h·1$ for the equation of light, we obtain 1795, May $11^d\ 7^h·1$, for the Paris heliocentric time of minimum. The times of minima indicated for the other observations are given with this in the first column of Table VI. The second column gives the ephemeris time for the epoch given in the third column. The last column gives the observed minus the computed times of minima.

TABLE VI.

Observed.	Computed.	Epoch.	O—C.
d h	d h		h
1795, May 11 7·1	May 11 11·9	−10980	−4·8
1795, May 18 8·5	May 18 11·5	−10977	−3·0
1797, May 22 12·3	May 22 21·8	−10661	−9·5

The mean of these results indicates a correction of six hours to the ephemeris, or of seven hours, if we assign somewhat greater weight to the last observation. A most fortunate coincidence brought all the observations so near minimum that the star had in each case less than its normal light. Obsérvations of the maximum light would have had comparatively little value.

The times of minima on May 11 and May 18, 1795, are somewhat uncertain, since the assumed light of the comparison stars may be in error. All the stars with which δ Libræ was compared have accordingly been arranged in sequences by Mr. Chandler, since the above reduction has been made. The details of these observations will be published elsewhere; but they give a correction for the minima of May 11th and May 18th of −2·3 and −2·5, instead of −4·8 and −3·0 hours. EDWARD C. PICKERING.

Markree Observatory.

[Continued from p. 288.]

MR. COOPER, about 1841, felt that it would be a great advantage to have a smaller instrument in the rough climate of Markree, where the sky clears so seldom and irregularly as to seriously interfere with a large instrument; accordingly, in 1842, he ordered a comet-seeker, mounted equatoreally, from Ertel. Its aperture is about 3 inches, and focal length 30 inches; the eyepieces magnified 12, 18, 27, and 40 times, the field being about 5° with the lowest power. When first mounted it was a dazzling object. The axes were placed accurately vertical to each other by the insertion of small pieces of strong paper, which checks vibration, and is therefore a great advantage in portable instruments; the same thing is often used also in large instruments. Micrometers and a lozenge-shaped aperture, movable from the eye-end (for photometric observations), were adapted to this instrument. It was soon found that the definition, when the higher powers were used, was only poor. On writing to Ertel for an explanation, it transpired that the instrument supplied was an excellent comet-seeker, but not adapted for micrometric work. It had been placed on a massive pier, in a small tower accessible from the revolving gallery of the refractor. Mr. Graham discovered the minor planet Metis with this instrument on 1848, April 25. It was dismounted in 1874, and a clock, visible to an observer at the refractor, erected in its place. I mounted the comet-seeker on a wooden stand, and found it then a most useful apparatus.

From time to time a collection of micrometers was added to the *matériel* of the observatory: the first were wire micrometers by Dollond and Gambez; but not satisfied with these, Mr. Cooper ordered one from Troughton, the wires of which, like those of Sir J. Herschel's, were found to slip; the circle, divided according to Sir W. Herschel's method of reckoning position-angles, was on the rim, which is very inconvenient. A fine double-image micrometer by Amici, in which the images are observed through glass prisms, the edges being turned to opposite sides, was acquired as early as 1832. In 1845 an excellent wire micrometer was purchased from Fraunhofer (an apparatus scarcely yet surpassed, though some form of ghost-micrometer may perhaps supersede it); the screw is a counterpart of Struve's and Dembowski's, and appears to be free from errors of any kind.

In observing the solar eclipse of 1836, May 15, Mr. Cooper, using dark glasses of a neutral tint, as he found red to give the worst definition, had to reduce the aperture to 5 inches. At Naples during the winter of 1844-45, Capocci lent him one of Melloni's water eyepieces, which enabled him to use the full aperture when observing the Sun with a power of 200; this induced him to get a similar eyepiece made in Dublin, in 1851. This eyepiece when in use weighs over 20 lb., the Sun being observed through a chamber containing a saturated solution of alum in rain water, which Sir J. Herschel considered merely a form of the plan adopted by his father, who mixed a little ink with the water, thus rendering the image colourless.

In order to fully utilize the observatory, which was described in the 'Monthly Notices' of the R. A. S. as the richest private observatory in the world, Mr. Cooper desired to secure the services of an assistant-astronomer.

In 1835, Hussey introduced to him Gambart, the well-known French astronomer, who was recommended by Bouvard, but died shortly after. Dr. Robinson sent him Mr. Robert Finlay, then a young man, an able mathematician, and afterwards Professor of Mathematics at Owen's College, Manchester, but who does not appear to have remained long at Markree. On the 1st March, 1842, Mr. Andrew Graham (born 1815, April 8, in the county Fermanagh), who had been trained to the use of the meridian-circle at Armagh, was appointed, and took up his residence at Markree.

With his arrival the activity of the Observatory got a new impetus. He lost no time in putting the meridian-circle into working order, and determined with great accuracy the latitude of

Markree, from stars of different declinations, observed both by direct and reflected vision. One of his principal works, in 1842 and 1843, was the determination of accurate positions of 50 telescopic stars within 2° of the pole. His results are reprinted in the Redhill catalogue. He also began to observe minor planets on the meridian, and persevered for many years in such observations, so that, subsequently, when working with the great refractor, he would often leave that instrument to secure a transit of a minor planet,—thus imposing upon himself the task, difficult under favourable circumstances, but much more so in the climate of Connaught, of keeping two large instruments in working order. He also computed many orbits and ephemerides of comets and minor planets. The difference of longitude between Dublin and Markree was determined by the aid of rockets in May 1839. In May 1842, rockets were fired by one of Dr. Robinson's sons from Mount Culcagh in Fermanagh, and observed at Armagh and Markree. The resulting longitude of Markree was $0^h\ 33^m\ 48^s{\cdot}38$ W. of Greenwich; exactly the same result was obtained in August 1847, from observations of three shooting-stars, by Mr. Cooper, at his house in Killiney, near Dublin, and Mr. Graham at Markree. The base was 98 miles.

Mr. Cooper always took a lively interest in cometary astronomy; he observed and sketched Halley's at its appearance in 1835, using the great refractor; but as the field was limited the comet-seeker was afterwards used, it being fitted with steel bars which were visible in a dark field.

In 1844–45 he travelled through France, Italy, and Germany (accompanied by Mr. Graham), making astronomical and meteorological observations *en route.* The great refractor was erected at Nice and at Naples on a wooden stand, with altitude and azimuth motions; and Mr. Cooper made a drawing of the Orion nebula, and independently discovered the third comet of 1844.

Subsequently Mr. Cooper, who was often absent from Markree, devoted himself to forming a catalogue of all the trustworthy cometic orbits that he could find in periodicals. This catalogue was published in Dublin in 1852, but meantime catalogues had been published by Jahn and Galle in 1847. It appears that Sir John Herschel had also begun a similar catalogue, but gave it up when he found himself forestalled. Mr. Cooper's catalogue frequently contains, however, valuable remarks not to be found in the other catalogues, particularly with reference to the physical appearance of comets. The preface contains some interesting statistics concerning the distribution of planetary and cometic orbits, a subject which always continued to engage his attention. Among his later investigations on comets, his sketches of Donati's comet, especially a moonlight view from the castle, deserve to be remembered.

In 1844 Mr. Graham began to check off all the stars in the 'Histoire Céleste' that were to be found in Piazzi's catalogue, intending afterwards to reobserve the remainder. He reduced their positions within $0^{m}\cdot1$ and 1' to 1845·0 and divided them into thirty zones. Mr. Cooper wrote to different observers requesting them to take zones. From Geneva, Padua, Modena, and Naples he was promised assistance, but Schumacher discouraged him. The project had its origin in the extraordinary delay in publishing Baily's reductions.

As long ago as the discovery of Astræa in 1845, Mr. Cooper was desirous of extending the Berlin star maps to stars of the 12th magnitude; but parts of the ecliptic not being included in these maps, he constructed new ones, to comprise stars found in any catalogue, and adopted a scale for the degree square 16 times as large. The zone observations were begun in 1847, Mr. Graham shortly afterwards suggesting the use of a square bar micrometer which he had invented and the theory of which he had worked out. It may be mentioned that, according to Dr. Robinson, the idea of using square bars was not entirely new, having been proposed by Wollaston long before; a micrometer of similar form was independently invented by Klinkerfues. A new eyepiece was constructed by Spencer in Dublin, enlarging the field of view of the refractor to nearly half a degree, the comet-seeker being thus superseded; it was, however, still used, and not without success, by the second assistant, Mr. Robertson. These observations were continued till 1860, June, when Mr. Graham resigned. The observations made during the years 1848–56 were, on the recommendation of the Royal Society, published in Dublin in four volumes, the expenses being defrayed by the Government. There were observations of upwards of 60,000 stars, situated within 3 degrees of the ecliptic, many being observed repeatedly. From a letter written by Mr. Graham in 1869, it appears that he continued the observations of the unfinished southern zones at the Cambridge Observatory, having found it then nearly impracticable at Markree; I hope he may be spared to publish the fifth volume. Unfortunately the maps were never published, but were presented to the Cambridge Observatory, after Mr. Cooper's death, by his daughters.

Mr. Cooper was elected a Fellow of the Royal Society in 1853, and in 1858 received the Cunningham Medal of the Royal Irish Academy, for the "Catalogue of Ecliptic Stars." He represented the county of Sligo in Parliament for many years. He died on 1863, April 23, shortly after losing his second wife, to whom he was deeply attached. His personal qualities were of a high order, and he was accomplished in many ways; but his zeal for science did not lead him to neglect the duties of his position, for he was a kind and good landlord, making great exertions to educate and improve his numerous tenantry.

After his death the Observatory remained uncared for till May 1, 1874, when I was appointed Director. I remained in charge for nine years, and was then succeeded by Mr. Albert Marth, the distinguished calculator.

W. Doberck,
Government Astronomer.

Hong-Kong Observatory.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The Eclipses mentioned by Thucydides.

SIR,—

In a letter to you last month, in reference to my contention that the eclipse, sometimes called that of Pericles, mentioned by Thucydides as having occurred in the first year of the Peloponnesian War was that of B.C. 431, August 3, rather than that of B.C. 433, March 30, Mr. Johnson apparently gives up either as the true date, and seems to favour an alteration of no less than twenty years in the accepted chronology of Grecian history, suggested by the late Rev. Franke Parker in a work published in 1858 under the title "Chronology."

Now this is not the place to enter into the general question of historical dates; but I may be permitted to point out that the alteration in question can be shown, by its effect upon all the eclipses mentioned by Thucydides, to be quite inadmissible. These were two of the Sun in the first and eighth years of the war respectively, and one of the Moon in the nineteenth.

The first of these, as already mentioned, I believe to have been that of B.C. 431, August 3. Mr. Parker proposes to substitute that of B.C. 451, March 20. My objection to that of B.C. 433, March 30, as having been too early in the season, from the number of events which took place in the campaign before the eclipse, applies even more strongly to this.

The second eclipse was probably that of B.C. 424, March 21. Mr. Parker would substitute that of B.C. 444, April 30. Although, in this case, no argument of much weight can be founded upon the season, yet, as the eclipse occurred at the very beginning of the campaign, the 21st of March is rather more suitable than the extreme end of April.

The third eclipse mentioned by Thucydides is the lunar eclipse which so alarmed Nicias in Sicily. The date generally accepted for this is B.C. 413, August 27, which was a total eclipse, and agrees therefore well with the circumstances. Mr. Parker's suggestion of B.C. 433, September 8, is quite inconsistent with them, as the eclipse which occurred then was only *penumbral.*

Let me conclude by drawing attention to an odd inadvertence of Mr. Parker. Although the B.C. dates in the Catalogue of ancient eclipses in 'L'Art de vérifier les Dates' are ordinary historical and not chronological, he alters even the dates suggested by himself from it each by one, and calls them *our* B.C. 450, 443, and 432. A still stranger inadvertence is fallen into by him in a note on "The Passover" at the end (p. 817) of his book. The year, he says, of the Death and Resurrection of Christ was "A.D. 33; that is, our A.D. 34." That it really was A.D. 33 according to any possible method of reckoning, I have very little doubt. Like Mr. Johnson, I believe that the lunar eclipse stated by Josephus as having occurred during the last illness of Herod the Great, a few months after the Nativity of Christ, was that of B.C. 1, January 9, not that of B.C. 4, March 12, as has been of late years generally supposed. Perhaps I may mention that I have given my reasons for considering the former to be the true date in 'Notes and Queries' for April 19th of the present year.

Yours faithfully,

Blackheath, 1884, Oct. 20. W. T. LYNN.

Conjunction of Venus.—Eclipse of Thales.

SIR,—

Allow me to correct an error on p. 227 of the 'Observatory' for August. It is in December 1886, not July, that the occultation of Venus by the Sun will take place. An inspection of the R.A. and Dec. of ♀ and ☉ on the morning of the 3rd of that month will show this.

There really seems no difficulty in assuming that Thales predicted the eclipse of B.C. 585 by means of the Chaldean Saros. If an eclipse of the Sun happens in the morning, it will sometimes be seen in the same country in the evening at the end of the period of 18 years, 11 days, $7\frac{3}{4}$ hours. To illustrate this by a few examples: a large solar eclipse seen in England in the early morning of July 1833 was also seen in July 1851, that of July 1842 in July 1860, that of May 1845 in May 1863, that of 1847 in 1865; not to mention the large eclipses of 1706 and 1715, seen in this country in the morning, returning after a Chaldean period so as to be visible in the same country in 1724 and 1733. To apply this to the case of Thales. There was a solar eclipse, visible over Asia Minor, on May 18, 603 B.C. Any one who saw or heard of this might venture to predict that, as the addition of the odd $7\frac{3}{4}$ hours would not throw the eclipse into the night, a similar phenomenon might be expected in the afternoon of May 28, 585 B.C.

Faithfully yours,

Melplash Vicarage, Bridport, Oct. 9. S. J. JOHNSON.

NOTES.

PARALLAX-HUNTING AT DUNSINK*.—For some years past Prof. Ball has employed the South refractor of the Dunsink Observatory in making a systematic search for stars with a large annual parallax. The third part of the *Observations* contains the results of measures of 42 objects selected for this purpose, and the volume now before us contains the observations of 368 additional objects.

In making out a working list of stars suitable for examination Prof. Ball has been guided by various considerations. Of course, stars with large proper motion would be naturally included, though the presumption of nearness founded on great proper motion can hardly be said to be justified by the results of observation. Many red stars and variable stars have been put on the working list. It has been suggested by Mr. Stoney that some of the former may

* Astronomical Observations and Researches made at Dunsink. Fifth Part. Dublin, 1884.

owe their colour to being very small, and therefore very close to us. There is also reason to believe that some of the variable stars are really very small, and that therefore, as we see them, they must be comparatively near us.

Several other stars have also been included which were chosen on different grounds.

In the reconnoitring observations carried out by Prof. Ball, every object is observed twice. The first observation is made when the star is at one of the extremities of the major axis of the parallax-ellipse, the second observation is made after an interval of six months, when the star has moved to that part of the parallax-ellipse which is at the other extremity of the major axis. The observation consists in the measurement of the distance and position-angle of the object under examination with regard to a suitable comparison star, which for the telescope and micrometer used should not be more than 300″ distant from the star whose parallax is to be examined, and not fainter than the 10th magnitude.

By comparing the two observations referred to above, it can be determined whether the object under examination has a parallax of any considerable amount or not. Of course it would have been preferable to make the observations at times when the effect of parallax on the relative positions of the star under examination and the comparison star would be greatest; but the determination of these times would have entailed some additional labour, and Prof. Ball has been satisfied with the simpler process here described.

Whenever the observations have suggested the existence of a measurable parallax, the objects have been observed again, and when necessary a complete series of observations has been made.

As might be expected, the results of the present work are mainly of a negative character, still the observations will be useful to future parallax seekers; and are also valuable as a small instalment of that complete survey of the heavens which is still a desideratum. The observations are, however, sufficient to enable us, in Prof. Ball's opinion, to assert that the parallax of these stars is certainly less than a second of arc, and most probably less than half a second of arc. It is of course possible that no star in the heavens has a parallax larger than a second. Prof. Ball somewhat naïvely remarks he often thinks that there certainly is not. It must, however, be remembered that comparatively very few stars have been examined with this object in view, and the induction is founded on really a very small number of instances. Prof. Ball's parallax-hunting is therefore a class of work which ought to be done, and it is most satisfactory to find that it is being done so skilfully and well.

In the present volume there are also included some positive fruits of Prof. Ball's work, viz. determinations of the parallax of 61 (B) Cygni (from differences of declination), of Groombridge 1618, and of 6 (B) Cygni*. The following are the results :—

* *Flamsteed's* 6 Cygni is B of that constellation; the star here referred to is Groombridge 2789, and the appellation 6 Cygni is taken from Bode.

Star.	Mag.	Parallax.	Mag. of Comparison star.
61 (B) Cygni	5·9	$+0''\cdot4676 \pm 0''\cdot0321$	9·5
Groom. 1618	6·8	$+0\ \cdot322\ \pm 0\ \cdot023$	8·8
6 (B) Cygni	6·5	$+0\ \cdot482\ \pm 0\ \cdot054$	10·5

These three stars have large proper motions.

As these determinations have, we believe, been previously published in scientific periodicals they need not be further alluded to here. We may, however, point out that the series of measures of 61 (B) Cygni alluded to on page 159 as having been made at Moscow between 1863, May 16, and 1866, Oct. 13, and published in 'Annales de l'Observatoire de Moscou,' vol. vii., 1 Livraison, page 79 *et seq.*, have been reduced by M. Socoloff (*loc. cit.* vol. viii., 2 Livraison), who finds—

From measures of distance, parallax of 61 Cygni	$= +0''\cdot4598 \pm 0\cdot0550$
From measures of position, parallax of 61 Cygni	$= +0\ \cdot4222 \pm 0\cdot0342$
From combination of measures, parallax of 61 Cygni	$= +0\ \cdot4330 \pm 0\cdot0291$

The observations were made by Schweizer, Struve's comparison star (9·4 mag.) being used. A. M. W. D.

The Total Lunar Eclipse of 1884, Oct. 4.—Up to the present time no method for determining the diameter of the Moon has been found wholly satisfactory. Observations of the bright limbs are liable to considerable errors from the effect of irradiation; and the method of occultations has not been much more successful, owing partly to the unevenness of the Moon's surface, the effect of which can only be counteracted by accumulating numerous observations at all points of its circumference, and partly to the disappearance and reappearance so seldom occurring under the same conditions of illumination of the Moon's limb. A total eclipse of the Moon offers, however, some peculiar advantages for the solution of this problem, since, from the diminution of the Moon's light, much fainter stars can be seen close to the limb than under ordinary circumstances, and therefore a much greater number of occultations can be observed. The disappearances and reappearances take place also under similar conditions, so that if as large a number of observatories as possible cooperate in the work, a better value for the apparent diameter of the Moon may be secured from the observations of a single night than can be obtained in the ordinary way from the labours of years.

Dr. Döllen of Pulkova having remarked that astronomers had hitherto neglected to avail themselves of these peculiar advantages offered by a lunar eclipse, sought to ensure that the eclipse of Oct. 4 should be better observed, and accordingly published, in No. 2615 of the 'Astronomische Nachrichten,' a list of 116 stars, with their positions, which would be occulted by the Moon; and

Prof. Struve forwarded to each of the principal observatories, from whence the eclipse could be observed, a list of the stars which would be occulted there, with the times and position-angles for immersion and emersion.

So far, however, as we have yet learned, the occultations observed have scarcely been so numerous as was anticipated. This was partly owing to the somewhat unusual appearance of the Moon during the eclipse. At Greenwich and several other observatories, the part of the Moon wholly in the shadow was invisible previous to totality, but the part in the penumbra was bright enough to overpower the faint stars, the occultation of which it was desired to observe; and during the total phase the Moon appeared unusually colourless and faint. The reports received from amateur astronomers in various parts of England are practically all to the same effect as regards this feature of the phenomenon. Canon Beechey, Downham, remarks:—"When in total penumbra I did not perceive the copper colour so often mentioned: it was, to my eye, a cold grey." "During totality it presented one equal flat tint of cold grey, through which every feature of the lunar surface was distinctly visible." He further adds that the eclipse was "remarkably similar to the one described by Smyth in the 'Prolegomena of his Celestial Cycle,' which occurred Oct. 13th, 1837." Mr. Hopkins, Leyton, "was much struck with the entire absence of the usual copper hue; the umbra appeared to me of an Indian-ink tint." Mr. Knight, Harestock, observed "that during totality the Moon appeared of a pure pale white, whereas during previous eclipses it has always been of a light claret-colour."

It was particularly unfortunate also that the Moon at the time of the eclipse was passing through a region barren of stars above the 9th and 10th magnitudes; great difficulty was experienced in following them, especially where the sky was at all hazy.

At Greenwich 37 observations were obtained with various instruments, six different stars being observed at immersion, nine at emersion. The telescopes employed were the Lassell reflector (2-ft.), the S.E. Equatoreal ($12\frac{3}{4}$-in.), Sheepshanks Equatoreal ($6\frac{3}{4}$-in.), Simms Equatoreal (6-in.), and a detached telescope (3·6-in.). Several English astronomers observed a number of the occultations and sent us their results, which have been forwarded to Prof. Struve.

Dr. E. Lindemann has kindly forwarded the following particulars of the results of observations of the eclipse. Cloudy weather prevented any observations being secured at no fewer than twenty stations, viz.:—

Armagh.	Karlsruhe.	Prague.	Dorpat.
Edinburgh.	Kiel.	Strasburg.	Pulkowa.
Glasgow.	Königsberg.	Vienna.	Riga.
Dresden.	Leipzig.	Padua.	Stockholm.
Göttingen.	Munich.	Charkow.	Upsala.

The following Table gives the stars observed at the more favoured stations:—

Occultations observed during the Total Lunar Eclipse, 1884, Oct. 4.

Observatory.	57	58	61	62	63	69	74	76	81	82	85	93	94	95	96	106	107	108	109	*
Brussels	...	...	E	E	E_2	E_2	E	...	I_2	I. E_2	I_2, E_2	...	I_2	I	I_2	...	...	...	...	1 I
Copenhagen	...	...	E	...	E	...	E	...	I	I	I, E	...	I							
Greenwich	...	...	E_2	E_2	I, E	E_3	E_3	I_2, E_2	I_2, E	E_3	I_2, E_3	...	I_3	I_4	...	...	...	...	I_3	
Oxford (Radcliffe)	23	obse	rvati	ons	of 13	stars,	by	two	obser	vers.	...	...	...	...	...	...	...	...	...	5 I
Bermerside	...	...	E	...	E	...	...	E	...	I	I	...	...	...	...	...	...	...	...	3 I
Colebyfield	...	...	...	...	...	E	E	...	I, E	...	E	...	I	I	...	...	...	...	I	
Bath	...	...	...	...	E	...	...	...	I	...	...	...	I	I	...	I	I	I	I	
Fernhill	...	...	...	...	I, E	E	E	...	...	...	I, E	...	...	I	...	...	...	...	I	
Paris	...	...	E_2	E	E_2	E	E	...	I_2	I_2, E_2	I_2, E	...	I_2	I_2						
Bordeaux	...	...	E	...	E	...	...	...	...	I, E	I, E_2	...	...	...	I	I_2	I	I		
Toulouse	...	...	...	...	E	...	E_2	...	I	...	I_2, E_2	...	I_3	I_3	I	I_2	I	I	I_3	
Marseilles	...	...	E_2	...	I_2	...	...	...	I_2, E_2	...	I_2, E	...	I_2	...	...	I	I	I_2	I_2	3 I
Lund	E	...	I, E	E	I_2, E_2	I_2	I_2, E_2	...	I_2, E	I, E	I	...	I	I	...	...	...	...	...	1 I
Gotha	4	obse	rvati	ons.																
Rome	...	...	E	...	E	...	...	...	I, E	...	I, E	I	I	I	...	...	...	I	I	1 E
Helsingfors	E	...	E	...	E	E	E	...	I	...	I									
Kasan	...	I, E	E	...	I, E	...	...	...	I											
Nikolajew	...	I, E	E	...	I, E	...	...	...	I	...	I, E									
Geneva	...	...	...	...	...	...	I	...	...	...	E	...	I	I	I	I				

The numbers at the heads of the columns refer to the stars in Dr. Döllen's list.
I signifies immersion; E, emersion; the small numeral attached gives the number of observations.
* The last column gives the number of anonymous stars observed.

The other observatories to which lists of the stars occulted were sent have as yet made no report.

M. Trépied observed the spectrum of the eclipsed Moon at Paris with a direct-vision spectroscope, keeping the slit normal to the outline of the shadow. He noted a continuous absorption spectrum from violet to orange, the orange and red being very faint. The spectrum from orange to violet was very similar to that presented by the nucleus of a faint comet, the colours being indistinguishable. MM. Paul and Prosper Henry took a number of photographs.

A set of 12 photographs, representing the various phases of the eclipse, has also been taken by the Rev. Thomas Perkins, M.A., who has presented them to the Liverpool Astronomical Society. These pictures differ from the telescopic aspect of the eclipse in showing a distinct penumbra, thus seeming to point to an absorption of the actinic, rather than of the visual, rays by our atmosphere.

We regret to learn from Dr. Lindemann that Prof. Struve has been suffering from ill-health recently, and has been confined to his room for the past three weeks.

Astronomy in America.—The following were among the astronomical subjects upon which papers were read at the Philadelphia Meeting of the American Academy of Arts and Sciences:—

The Asteroid Ring.—Prof. M. W. Harrington, director of the Ann Arbor Observatory, Michigan, read a paper upon the asteroid ring. The representative average orbit seemed to be an ellipse of small eccentricity, with semimajor axis equal to about 2·7 times that of the Earth, and inclined to the plane of the ecliptic about 1°. In the progressive discovery of these small bodies, the average mean distance had gradually increased, but now seemed to have reached its limit. On the assumption that the surfaces of all the asteroids have the same reflecting power as Vesta, Prof. Harrington concludes that the volume of Vesta is about $\frac{5}{17}$ that of the whole of the first 230, and that Vesta and Ceres together form almost one half the total volume.

Temporary Stars.—Prof. Daniel Kirkwood, in discussing the question whether the so-called "temporary stars" may be variables of long period, referred to the supposed identity of the temporary stars of 945 and 1264 with the well-known star of Tycho Brahé in Cassiopeia in 1572, and the position of which is pretty closely known from his measures. He considers that, on account of the sudden apparition of the temporary stars, the short duration of their brightness, and the extraordinary length of their supposed periods, they should be considered as distinct from variables.

The Almacantar.—Mr. S. C. Chandler, jun., gave the results of observations and experiments with the Almacantar, described in the 'Observatory,' No. 78, p. 305. Some small variations in zenith-distance pointing were traced to thermal changes, and were completely overcome by sawing through the wooden bottom of the mercury-trough; the constancy of the zenith-point now exceeds that obtained with the best fundamental instruments of large observatories.

Colour of Variable Stars.—In a second paper, On the Colour of Variable Stars, Mr. Chandler stated that, in addition to the two laws already known—viz. 1, that these stars are generally red; 2, that they increase in brightness more rapidly than they decrease—a third law must have a place in any new theory, viz. the redder the star, the longer is the period.

Nebulæ.—Prof. Lewis Swift described his method of searching for new nebulæ, and of simply recording their approximate positions by pointing with illuminated cross wires in the eyepiece, and reading off the circles of the instrument, recording at the same time a description of the appearance of the nebula. He stated that there had not been a first-rate clear sky since the red glows were first visible about a year ago, a statement corroborated by several American observers.

An interesting discussion arose as to the much disputed existence of the nebula round the star Merope in the Pleiades, the general drift of it being that the nebula no doubt existed; but in order to see it a clear sky was necessary, and a very low power and large field, so that the nebula might be contrasted with the darker portions of the same field; that a large telescope was not necessary, in fact the smaller the better, provided the optical qualities were relatively as good. Mr. Swift said he could always see it under favourable conditions; and Mr. E. E. Barnard, of Nashville, Tenn., said that before knowing of its existence, he picked it up as a supposed comet.

McCormick Observatory.—Prof. Ormond Stone gave a description of the McCormick Observatory, Virginia, which is now approaching completion, and which is to be entirely devoted to original research. The telescope, which will soon be mounted, is twenty-six inches in aperture. The dome, which is 45 feet in diameter and hemispherical in shape, is of very ingenious construction. It weighs 12½ tons, and a tangential pressure of 40 lb. or 8 lb. on the endless rope is sufficient to start it. The first work proposed is the remeasurement of double stars of less than 2″ distance from 0° to 30° S. declination.

Value of the Obliquity of the Ecliptic.—Professor J. C. Adams read a paper upon the general expression for the value of the obliquity of the ecliptic at any given time, taking into account terms of the second order. The difficulties of obtaining a formula for this quantity, on account of the many varying elements upon which it depends, were clearly explained by a diagram, and the results given of an approximation carried much further than heretofore attempted.

Prof. Simon Newcomb, in reference to the value of the Moon's mass used in the expression, considered that it could be obtained most accurately by observations of the Sun, in determining the angular value of the radius of the small circle described by the Earth about the common centre of gravity of Earth and Moon, since this, in his opinion, seemed to be the only constant which could be determined by observation absolutely free from systematic errors, and hence was capable of an indefinite degree of accuracy by accumulated observations; though admitting, in the case of *absolute* determinations, the impossibility of measuring such small quantities, he ventured in the case of *differential* or *relative* determinations, in which there was no supposed possibility of constant or systematic errors, to advance the theory, which he had thought of elaborating more fully at some time, that such determinations might be carried by accumulated observations to a sure degree of accuracy far beyond what can be seen or measured by the eye absolutely.

Newton's Theory of Refraction.—Professor Adams also presented another note upon Newton's theory of atmospheric refraction, and on his method of finding the motion of the Moon's apogee. He described in an exceedingly interesting manner how some unpublished manuscripts of the great geometer had lately come into his hands at Cambridge, which contained later work than is published in the 'Principia,' and exhibited photographs of the papers.

The Prime Meridian Conference.—The Conference which has met at Washington to decide on the questions of a prime meridian and universal time have adopted the meridian of Greenwich as the initial one, the mean solar day for the universal day, and mean midnight at Greenwich as the commencement of the day. Longitude is to be reckoned from zero up to twenty-four hours.

Annals of Harvard College Observatory, Vol. XIV.—Part. I of the Annals of this Observatory, containing the observations with the Meridian Photometer, has just been published. Some idea of the work may be formed when it is stated that the results are based upon about 100,000 separate comparisons, extending from 1879, Oct. 25, to 1882, Sept. 17. Prof. Pickering considers that all stars down to the sixth magnitude are included, unless they have been underestimated in the several standard catalogues adopted for the formation of the working list. The bulk of the volume is occupied by the General Catalogue, giving the magnitudes of 4260 stars as determined by the meridian photometer, and comparisons of these with those given by fourteen standard authorities from Ptolemy to the present time. We trust shortly to notice more fully this valuable and important work.

Spectrum of Comet 1884 *b* (Barnard)*.—M. Perrotin at Nice succeeded, on Sept. 16, 17, and 18, in detecting two of the three ordinary cometary bands in the spectrum of this comet, and believed that he could occasionally glimpse the third, the one in the blue. The bands were much stronger over the nucleus. The nucleus in the telescope appeared very distinct, and of a squat and granular appearance. It was from 10″ to 12″ of arc in diameter

* Comptes Rendus, Vol. xcix. No. 13.

and was prolonged by a thin bright thread in position-angle 50°. The coma was about 2′·5 in diameter, and extended slightly in the same direction, spreading out in the shape of a fan.

A New Minor Planet, No. 243, was discovered on Sept. 29, and another, No. 244, on Oct. 14, both by Herr Palisa of Vienna. No. 241 has received the name Germania. The elements of its orbit resemble those of No. 133, Cyrene: but Dr. Luther has made it evident that it is a different body, by observing the latter on several occasions recently. "The orbit elements," he adds, "are indeed similar, but differ throughout."

On the suggestion of a subscriber, we have had the diagram of the orbits of the seven inner satellites of Saturn, given on p. 272, printed on stiff cardboard, varnished and taped for hanging up in observatories. They may be procured, price sixpence each, from the Editor or the Publishers.

Prof. Asaph Hall informs us that "by observations made at Washington, Sept. 20 to Sept. 25, Mr. Marth's Ephemerides of Mimas and Hyperion for 1884 are nearly correct."

Elliptic Elements of Comet 1884 *b* (*Barnard*)*.

By Prof. J. Morrison, M.D.

T = 1884 Aug. 16·29127, Washington M.T.

$\pi - ☊$	301° 25′ 45″·45	Mean Eq. 1884·0.	
☊	4 11 2 ·71		
i	5 39 3 ·14		
log q	0·1116397	ϕ	38° 45′ 45″·19
a	3·45482	Period ..	6·43157 years.

Dr. Berberich, of Strasburg, has also calculated elliptic elements for this comet, and obtains a period of 5·4965 years. If Dr. Berberich's period is correct, the comet at aphelion is distant from the orbit of Jupiter 0·503, but approaches Mars much more closely, viz. 0·0088, at heliocentric longitude 343° 25′, and both Mars and the comet would have passed this point between April 5 and 10, 1868.

Elliptic Elements of Comet 1884 *c* (*Wolf*).

This comet has also proved to be one of short period, the revolution time being about six and a half years. Elliptic elements have been already supplied by several calculators; the following are from the 'Astron. Nachr.' No. 2623, and are by Dr. K. Zelbr.

T = 1884 Nov. 17·7111, Berlin M.T.

π	18° 56′ 28″·3	Mean Eq. 1884·0.	
☊	206 21 5 ·8		
i	25 16 48 ·5		
log q	0·196893	ϕ	34° 2′ 52″·8
log a	0·553330	μ	524″·822

* Dun-Echt Circular, No. 92.

Maxima and Minima of Variable Stars in 1884, November.

M, signifies maximum ; *m*, minimum.

Nov.	1·9	ζ Geminorum, M.
	3	R Arietis, *m*.
	5	S Herculis, *m*.
	6	R Ophiuchi, M.
	6·2	β Lyræ, *m*.
	7	R Lyræ, *m*.
	7·1	ζ Geminorum, *m*.
	7·2	W Virginis, *m*.
	7·5	T Monocerotis, *m*.
	12	T Ophiuchi, M.
	12	S Virginis, M.
	12·1	ζ Geminorum, M.
	13	R Andromedæ, M.
	14	T Herculis, M.
	15	S Coronæ, *m*.
	15	R Aquilæ, M.
	15	χ Cygni, M.
Nov.	15·4	W Virginis, M.
	15·5	T Monocerotis, M.
	17	R Vulpeculæ, *m*.
	17·2	ζ Geminorum, *m*.
	19	R Virginis, *m*.
	19·1	β Lyræ, *m*.
	20	U Monocerotis, *m*.
	22	R Lyræ, M.
	22	S Tauri, M.
	22·2	ζ Geminorum, M.
	23	S Vulpeculæ, *m*.
	24	R Sagittæ, *m*.
	24·4	W Virginis, *m*.
	25	U Virginis, *m*.
	27	S Piscium, M.
	27·4	ζ Geminorum, *m*.

Variables of Short Period.

δ Cephei.

	d.
Nov.	2·85, *m*.
	4·45, M.
	8·25, *m*.
	9·85, M.
	13·55, *m*.
	20·55, M.
	24·35, *m*.
	29·65, *m*.

W Sagittarii.

	d.
Nov.	2·3, *m*
	5·4, M.
	9·9, *m*.
	17·5, *m*.
	20·6, M.

U Sagittarii.

	d.
Nov.	8·8, *m*.
	11·7, M.
	15·5, *m*.
	18·5, M.
	22·3, *m*.
	25·2, M.

η Aquilæ.

	d.
Nov.	4·7, *m*.
	14·3, M.
	21·4, M.
	26·2, *m*.
	28·6, M.

X Sagittarii.

	d.
Nov.	3·7, *m*.
	6·6, M.
	10·7, *m*.
	13·6, M.
	17·7, *m*.
	20·6, M.
	24·7, *m*.
	27·6, M.

U Cephei, *m*.

		h	m
Nov.	2	6	41
	4	18	31
	7	6	21
	9	18	10
	12	6	0
	14	17	50
	17	5	40
	19	17	29
	22	5	19
	24	17	9
	27	4	59
	29	16	48

Algol, *m*.

		h	m
Nov.	3	18	30
	6	15	19
	9	12	8
	12	8	57
	15	5	46
	26	17	1
	29	13	50

λ Tauri, *m*.

		h	m
Nov.	24	18	13
	28	17	5

S Cancri, *m*.

		h	m
Nov.	8	8	27
	27	7	42

δ Libræ, *m*.

		h	m
Nov.	28	15	12

U Coronæ, *m*.

		h	m
Nov.	3	13	47
	10	11	30
	17	9	12
	24	6	55
	27	17	46

*Ephemeris for Comet 1884 c (Wolf)**.
By Dr. A. Krueger.

Berlin Midnight.	R.A. h m s	Dec. ° ′	Log r.	Log Δ.
Oct. 29....	22 4 50	+4 10·8	0·1982	9·9340
31....	22 8 56	+3 22·9		
Nov. 2....	22 13 8	+2 36·8	0·1970	9·9435
4....	22 17 26	+1 52·5		
6....	22 21 50	+1 10·1	0·1960	9·9539
8....	22 26 20	+0 29·7		
10....	22 30 55	−0 8·8	0·1953	9·9650
12....	22 35 34	−0 45·3		
14....	22 40 17	−1 19·8	0·1949	9·9768
16....	22 45 4	−1 52·2		
18....	22 49 56	−2 22·6	0·1948	9·9892
20....	22 54 52	−2 51·0		
22....	22 59 50	−3 17·3	0·1950	0·0020
24....	23 4 50	−3 41·6		
26....	23 9 52	−4 4·0	0·1955	0·0151
28....	23 14 56	−4 24·4		
30....	23 20 2	−4 43·0	0·1962	0·0286

Differences of Right Ascension and Declination between Iapetus and the Centre of Saturn. By A. Marth.

Iapetus – Saturn.

Greenwich Noon.	R.A. s	Dec. ″	Greenwich Noon.	R.A. s	Dec. ″
Nov. 1....	+35·3	+ 17	Nov. 16....	+37·0	+189
2....	37·1	32	17....	35·2	194
3....	38·7	46	18....	33·1	198
4....	40·1	61	19....	30·9	200
5....	41·2	75	20....	28·4	201
6....	42·1	89	21....	25·7	201
7....	42·7	103	22....	22·9	200
8....	43·1	115	23....	19·9	197
9....	43·2	128	24....	16·8	194
10....	43·1	139	25....	13·6	189
11....	42·7	150	26....	10·3	182
12....	42·1	160	27....	7·0	175
13....	41·2	169	28....	3·6	166
14....	40·0	177	29....	+ 0·1	156
15....	+38·7	+181	30....	− 3·3	+146

* Astron. Nachr. No. 2619 and 2622.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

Mimas (East).		h
Nov.	17	16·8
	18	15·4
	19	14·0
	20	12·6
	21	11·2
	22	9·8
	23	8·4
	24	7·0

Mimas (West).		
Nov.	9	16·6
	10	15·2
	11	13·8
	12	12·4
	13	11·0
	14	9·6
	15	8·2
	16	6·8
	25	17·0
	26	15·6
	27	14·2
	28	12·8
	29	11·4

Enceladus.		h
Nov.	1	23·8
	3	8·6
	4	17·5
	6	2·4
	7	11·2
	8	20·1
	10	5·0
	11	13·9
	12	22·8
	14	7·7
	15	16·6
	17	1·4
	18	10·3
	19	19·2
	21	4·0
	22	12·9
	23	21·8
	25	6·7
	26	15·6
	28	0·4
	29	9·3
	30	18·1

Hyperion.		
Nov.	8	2·7
	29	6·9

Tethys.		h
Nov.	1	3·9
	3	1·2
	4	22·5
	6	19·8
	8	17·1
	10	14·5
	12	11·7
	14	9·0
	16	6·3
	18	3·5
	20	0·8
	21	22·1
	23	19·4
	25	16·7
	27	14·0
	29	11·3

Titan.		
Nov.	1	12·0
	17	9·0
Dec.	3	7·1

Dione.		h
Nov.	2	4·0
	4	21·7
	7	15·3
	10	8·9
	13	2·5
	15	20·2
	18	13·8
	21	7·5
	24	1·1
	26	18·8
	29	12·4

Rhea.		
Nov.	1	12·5
	6	·09
	10	13·2
	15	1·5
	19	13·9
	24	2·1
	28	14·5

The following table will furnish the means for inferring roughly the distances and position-angles of the satellites from the diagram given on page 272.

		Position-angle of Minor Axis of Ring.			Semi-major axis of orbit of Titan.
Oct.	30	354° 18′	Oct.	30	202″·8
Nov.	9	354 21	Nov.	9	205 ·4
	19	354 25		19	207 ·4
	29	354 30		29	208 ·6

Astronomical Memoranda, 1884, November.

Equation of Time:—Sun *before* Clock, Nov. 1, $16^m\ 19^s$; Nov. 11, $15^m\ 46^s$; Nov. 21, $13^m\ 49^s$; Nov. 30, $10^m\ 56^s$.

Sidereal Time at Mean Noon:—Nov. 1, $14^h\ 44^m\ 42^s$; Nov. 11, $15^h\ 24^m\ 8^s$; Nov. 21, $16^h\ 3^m\ 34^s$; Nov. 30, $16^h\ 39^m\ 3^s$.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Nov. 2....	19 0	4 29	24 19	+4 0	47 51
7....	19 8	4 21	23 18	3 27	341 56
12....	19 16	4 13	22 4	2 53	276 0
17....	19 25	4 6	20 39	2 17	210 6
22....	19 34	4 0	19 4	1 40	144 13
27....	19 41	3 55	17 18	1 3	78 19

The position-angles of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon.

Moon.	*sets.*		*rises.*		*sets.*
	h m		h m		h m
Nov. 1..	17 26	Nov. 11..	13 22	Nov. 21..	7 17
2..	*rises*	12..	14 30	22..	8 14
3..	4 55	13..	15 37	23..	9 14
4..	5 39	14..	16 42	24..	10 17
5..	6 31	15..	17 46	25..	11 23
6..	7 32	16..	*sets*	26..	12 32
7..	8 39	17..	4 20	27..	13 44
8..	9 49	18..	4 56	28..	14 59
9..	11 1	19..	5 38	29..	16 15
10..	12 12	20..	6 25	30..	17 33

Full Moon, Nov. 2, $20^h\ 37^m$; Last Quarter, Nov. 9, $11^h\ 12^m$; New Moon, Nov. 17, $6^h\ 12^m$; First Quarter, Nov. 25, $10^h\ 16^m$.

Mercury in superior conjunction Nov. $4^d\ 8^h$.

Venus a morning star in Virgo, in conjunction with the Moon Nov. $13^d\ 18^h$. Diameter:—Nov. 1, 16″·5; Nov. 30, 13″·8. Illuminated portion of disk 0·738 on Nov. 15.

Nov. 1,	R.A. $11^h\ 52^m{\cdot}6$,	Dec. 2° 17′ N.,	tr. $21^h\ 8^m$,	rises $14^h\ 55^m$
30,	14 2 ·1,	10 17 S.,	21 24	16 15

Mars an evening star, in conjunction with the Moon Nov. $19^d\ 2^h$. Diameter:—Nov. 1, 4″·8; Nov. 30, 4″·8. Illuminated portion of disk 0·984 on Nov. 15.

Nov. 1,	R.A. $16^h\ 10^m{\cdot}5$,	Dec. 21° 43′ S.,	tr. $1^h\ 26^m$,	sets $5^h\ 31^m$
30,	17 43 ·4,	24 15 S.,	1 4	4 52

Jupiter in Leo, in conjunction with the Moon Nov. $10^d\ 18^h$; in quadrature Nov. $26^d\ 5^h$. Diameter:—Nov. 1, 32″·6; Nov. 30, 35″·4.

Nov. 1,	R.A. $10^h\ 17^m{\cdot}8$,	Dec. 11° 30′ N.,	tr. $19^h\ 30^m$,	rises $12^h\ 28^m$
30,	10 29 ·4,	10 21 N.,	17 48	10 50

Saturn, in conjunction with the Moon Nov. $5^d\ 15^h$.

Nov. 1,	R.A. $5^h\ 32^m{\cdot}5$,	Dec. 21° 48′ N.,	tr. $14^h\ 45^m$,	rises $6^h\ 41^m$
30,	5 24 ·3,	21 42 N.,	12 43	4 39

	Outer Ring. Maj. Axis.	Min. Axis.	Inner Ring. Maj. Axis.	Min. Axis.	Ba… Di…
Nov. 9......	45″·66	20″·41	30″·36	13″·57	18…
29......	46 ·44	20 ·80	30 ·88	13 ·83	18…

The south side of the rings is visible, the elevation of the Earth ove their plane on Nov. 9 being 26° 33′ S., and on Nov. 29 5° 37′ S., and of the Sun 26° 48′ S. and 26° 49′ S.

Uranus.

	R.A.	Dec.	tr.	rises
Nov. 1,	$12^h\ 4^m \cdot 9$,	0° 15′ N.,	$21^h\ 17^m$,	$15^h\ 12^m$
30,	12 9 ·6,	0 15 S.,	19 27	13 22

Neptune in opposition Nov. $13^d\ 9^h$.

	R.A.	Dec.	tr.	rises
Nov. 1,	$3^h\ 21^m \cdot 4$,	16° 36′ N.,	$12^h\ 34^m$,	$5^h\ 2^m$
30,	3 18 ·1,	16 24 N.,	10 37	3 7

Phenomena.

	G. M. T. h	m	
Nov. 1	14	3	J. i. Ec. D.
	17	31	J. i. Oc. R.
2	17	54	31 Arietis Oc. D. 127°.
	14	41	J. i. Tr. E.
	16	55	J. iv. Tr. I.
4	10	38	B.A.C. 1351 Oc. R. 311°.
	10	44	63 Tauri Oc. R. 279°.
	14	32	J. iii. Tr. I.
	18	13	J. iii. Tr. E.
5	10	46	115 Tauri Oc. R. 233°.
6	15	39	J. ii. Ec. D.
7	12	14	68 Geminorum Oc. R. 276°.
8	13	4	J. ii. Tr. I.
	13	18	B.A.C. 2872 Oc. R. 252°.
	15	56	J. i. Ec. D.
	16	0	J. ii. Tr. E.
	18	38	A¹ Cancri Oc. R. 243°.
9	14	17	J. i. Tr. I.
	15	5	*h* Leonis Oc. R. 248°.
	16	37	J. i. Tr. E.
10	13	55	J. i. Oc. R.
	16	13	J. iv. Ec. D.
Nov. 11	17	19	76 Leonis Oc. R. 175°.
13	18	15	J. ii. Ec. D.
15	15	41	J. ii. Tr. I.
	17	49	J. i. Ec. D.
16	16	12	J. i. Tr. I.
17	12	59	J. ii. Oc. R.
	15	50	J. i. Oc. R.
18	13	0	J. i. Tr. E.
19	16	24	J. iv. Tr. E.
22	12	49	J. iii. Oc. D.
	16	28	J. iii. Oc. R.
	18	16	J. ii. Tr. I.
23	18	6	J. i. Tr. I.
24	14	10	J. i. Ec. D.
	15	36	J. ii. Oc. R.
	17	43	J. i. Oc. R.
25	5	39	θ Aquarii Oc. D. 138°.
	6	52	θ Aquarii Oc. R. 270°.
	12	35	J. i. Tr. I.
	14	54	J. i. Tr. E.
26	12	11	J. i. Oc. R.
27	4	19	B.A.C. 8311 Oc. D. 134°.
	14	55	J. iv. Ec. R.
29	15	19	J. iii. Ec. R.
	16	47	J. iii. Oc. D.
30	6	35	38 Arietis Oc. D. 75°.

The angles are reckoned from the *apparent* N. point towards the ght of the Moon's inverted image.

EDITOR.

ERRATUM in No. 90, p. 284, line 17, *for* reflector *read* refractor.

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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 92. DECEMBER 1. 1884.

EDITORIAL ADDRESS.

With the present Number The Observatory completes its seventh volume. Started in April 1877 by the present Astronomer Royal, then Chief Assistant at Greenwich Observatory, it speedily became apparent that, under his able management, it supplied a want not adequately met by any other astronomical periodical; and it has formed an almost indispensable supplement to the 'Monthly Notices.' The undiminished support which has been generously extended to the present Editor would appear to indicate that in his hands it has not ceased to be useful; but he himself has found it increasingly difficult, in the scanty leisure of an Assistant of Greenwich Observatory, to properly secure its continued efficiency unaided. It is therefore with the greatest pleasure that he begs to announce that Mr. A. M. W. Downing, M.A., Member of the Council of the Royal Astronomical Society, and well and widely known by the many and valuable papers he has communicated to that Society, and Mr. T. Lewis, Assistant in charge of the Time Department at the Royal Observatory, have consented to share the duties and responsibilities of the editorial office with him, and that with the new year the 'Observatory' will bear the three united names.

The Editors will use their utmost endeavours to secure and increase the usefulness and interest of the magazine; and with that view propose, whilst maintaining all the present features of the publication, to give greater attention to two departments which, from the nature of the case, cannot be supplied by the 'Monthly Notices' of the Royal Astronomical Society. They will endeavour, in the first place, to review at adequate length every astronomical work of high importance as it appears; and, in the second, to assist amateurs just commencing astronomical observation, by the publication, from time to time, of papers by practical workers descriptive of various departments of work. It is intended to attempt to make the "NOTES" serve the purpose of a monthly review of the progress of astronomy still more fully than in the past; and to occasionally supplement them by an account, historic and descriptive, of some important Observatory. The Editors trust that by these means they may yet further extend its field of usefulness; and they also hope that astronomers, to many of whom the 'Observatory' has been so greatly indebted in the past, will still continue to communicate articles and letters on miscellaneous matters of interest connected with the science.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, 1884, November 14.

E. DUNKIN, F.R.S., *President*, in the Chair.
Secretaries: Lieut.-Col. G. L. TUPMAN, and
A. M. W. DOWNING, M.A.

THE Minutes of the last Meeting were confirmed.

Col. Tupman announced that 159 presents had been received since the last Meeting. One of these, calling for very special notice, is the enormous Catalogue of Southern Zone Stars by Dr. Gould. It is the most stupendous work of its kind that has appeared for a long time. There are some 73,000 stars, and many of these have been observed two or three times.

The President. The stars are arranged in order of right ascension, as in ordinary catalogues, and the places are thus available for astronomers. I think this is one of the greatest contributions made to astronomy during the last ten years.

Col. Tupman. There is also a work of a more original kind. It is Dr. Pickering's Photometric Measures of upwards of 4200 Stars, a work of equal magnitude with the last mentioned. We have also a wonderful photograph of the Cluster in Perseus by the Brothers Henry of Paris, taken with an object-glass of ·16 metre, focal length 2·1 metres, 50 minutes exposure, and enlarged three times. There is a wonderful degree of sharpness, so much so, as to have the appearance of a chart. Another present was a maximum and minimum thermometer by Dr. Spitta of Clapham. It is his own invention, and seems an improvement on Six's thermometer.

Dr. Spitta. It has three limbs instead of two, one being a compensating limb. It is entirely different from the ordinary thermometer.

The thanks of the Society were unanimously voted to the respective donors.

Prof. Pritchard read a paper "On the Photometric comparison of Light transmitted by certain Refracting and Reflecting Telescopes of equal Aperture." He said that among the stars occulted by the Moon in the recent total eclipse, there were observed at Oxford four faint stars not in Dr. Döllen's list and not found in any catalogue. One only of these stars was seen in the De-la-Rue reflector of 13 inches aperture, while all were observed, during the eclipse, by the Grubb refractor of 12¼ inches aperture, and were afterwards identified with the same instrument. His attention was thus drawn to the question of the relative light capacities of refracting and reflecting telescopes of nearly equal aperture. He found that the received opinions were purely conjectural. Dr. Robinson, the greatest authority on the subject, says in the 'Monthly Notices,' May 1876, "W. Struve *thought* that the Dorpat telescope, 9·6 inches aperture, might be compared with Herschel's 20-feet reflector of 18 inches aperture;" and he also adds, "We are told that D'Arrest's achromatic of 11 inches aperture is very superior to Herschel's 20-feet telescope, and almost equal to the great reflector of Lord Ross."

He believed that the British Association had appointed a committee to inquire into the subject; but he could not find that they had published any report. The observatory at Oxford under his direction possessed a photometer especially adapted to solve the question, and through the generosity of Dr. De la Rue it also possesses two mirrors of nearly equal dimensions, one of metal, the other of silver on glass by With. A list of stars from the 2nd to 6th magnitude was prepared, and Messrs. Plummer and Jenkins made the observations with the Grubb refractor and the two reflectors, all about the same aperture, as follows:—Each observer, armed with his own wedge-photometer and its eyepiece attached, noted the point of extinction of each selected star. Five extinctions of each star were observed in each instrument, the mean adopted, and the relative light capacities computed, as indicated in the 47th volume of the Society's Memoirs. The result is as follows:—The light transmitted by the Grubb refractor (12¼-inch) is to the light transmitted by the De-la-Rue reflector (13-inch) as 1·89 to 1; the light transmitted by the Grubb refractor is to that transmitted by the silver-on-glass mirror (13-inch) as 1·5 to 1. The average deviation of the individual measures from the adopted mean does not exceed the light indicated by $\frac{1}{20}$th of what we call a magnitude on Pogson's scale; in other words, as much as the human eye can distinguish. I have not entered upon the general question of comparing glass with mirrors, whether of metal or deposited silver, but have confined the question to the large telescopes with which I am necessarily familiar, though I have little doubt that they may be regarded as representative instruments. Still less am I pretending to decide as to the relative superiority of the two forms of telescopes. There are, however, two important remarks made by the late Dr. Robinson in the 'Monthly Notices' to which I am disposed to give my entire assent. The one is that in proportion as the aperture of the two sorts of instruments are greatly increased the superiority of the light-transmitting power of the refractor becomes less and less; and the other is that few persons are probably aware of the considerable loss of light in its passage through the object-glass on account of the superficial reflection and the absorption of light by material. This loss of light increases in geometrical ratio with the thickness of the glass, and the augmentation of thickness necessarily increases at a rapid rate with any increase of aperture. From this peculiar idiosyncrasy reflectors are free; and if it were possible sufficiently to increase the aperture of the object-glass there must be at length arrived at a thickness of glass material which will absorb more light than is lost by the reflector on the speculum. Probably, however, it is practically impossible for an object-glass of such a size to be manufactured and figured; whereas the possible limit of the aperture of a reflecting power at the present time is not only not attained, but not even, as yet, suspected. I have to agree consequently with the late eminent astronomer of Armagh, that attention may in the future be directed to the increase of aperture and to the mirrors, whether parabolic or plane, with the extension of our knowledge in astronomical physics.

Mr. Ranyard. What diameter is the silver-on-glass speculum?

Prof. Pritchard. They are all about the same—13 inches approximately. The $12\frac{1}{4}$- and the 13-inch silver-on-glass speculum are comparable. I may only quote the concluding remarks by Dr. Robinson in the 'Monthly Notices' already cited, to the effect that comparative measures as are here instituted can only be effectually made where the two forms of instruments are set up side by side, and the comparisons can only be fairly made when the conditions are identical, and when the locality, the light, the object, and the observer are the same, and I may add when there is no personal or party bias in the judge. I trust these conditions are well secured in the researches detailed in the present communication. (Applause.) The instruments were worked side by side, the conditions were the same, they are the same stars, and the observations were made by two utterly unbiassed men who had no communication one with the other, and they came to the same result. In a future communication I hope to give the results of photometric inquiry into the amount of light absorbed and scattered by various object-glasses and by glasses in general of various descriptions and various thicknesses.

Lord Crawford asked if the reflector was of the Herschelian or Newtonian form.

Prof. Pritchard. Of the Newtonian; but this is not an inquiry what light is lost by the *mirrors* but by the *telescope.*

Lord Crawford. There are two mirrors instead of one, and you can use it in another way—for instance, for photography.

Prof. Pritchard. You must permit me to do what I can; I have merely attacked the question as to the telescopes, and not as to their photographic power.

Mr. Brett asked if the photometer was put in the principal focus, and if a different eyepiece was used for each telescope.

Prof. Pritchard. The photometer carries the eyepiece with it.

Mr. Common inquired at what part of the cone of rays of light the dark wedge was placed.

Prof. Pritchard. We pursued the same course as we did with our photometry; but wherever you put the wedge, it will not prejudice the result. The way it is done is this:—There is what used to be called a Dawes's eyepiece, a little diaphragm with various holes in it, and that is placed in the focus of the telescope, the mirror, or object-glass. The star's image is formed well within that hole, and in front of that is put the wedge which slides until it obscures the light, and by a little mathematical business we derive the relative intensity of the light.

Mr. Common. I am afraid I have not made myself perfectly clear.

[*Mr. Common* drew a diagram on the blackboard showing the course of the rays near the primary focus of the telescope, and asked at what part of the cone the wedge was placed, and this was pointed out by Professor Pritchard.]

Prof. Pritchard. Without entering on these points, I believe the thing is perfect in its way. There is a limit to the power of observation. After all, it is an appeal to the power of the eye to determine when two lights are equal. Now when in a totally different instrument Professor Pickering gets absolutely the same results, they must be accurate.

Mr. Common. May I ask you to put in your paper the actual means employed, so as to give sufficient data for any person to repeat the experiment and criticize it fully?

Prof. Pritchard. Alas for one's immortality! Every single thing is put down—the method, the measures, and every thing about it—in the last Memoirs of the Society, and you have not read it. Why, I thought I had rendered myself immortal! (Laughter.)

Mr. De la Rue. I rise first of all to express my entire confidence in the comparative measures made by Prof. Pritchard, as the results I have obtained of the relative amount of light transmitted by refracting and reflecting telescopes coincide very much with the numbers that Prof. Pritchard has given. Lord Crawford has pointed out that the comparison does not hold good for photography, as we have only one reflection then; but we have two in these eye observations. I would also add that the photometric comparison between an ordinary speculum metal reflector and one with silver on glass does not hold good for photography, for the speculum metal reflector reflects more actinic rays than the silver on glass does, a great number of actinic rays being transmitted through silver.

Mr. Ranyard. Before taking Prof. Pritchard's paper as affording a standard of comparison, I should like to know how long ago the silver was deposited, and whether the silver was in its brightest condition both upon the flat and upon the mirror. If this was not the case it would entirely upset the belief hitherto existing as to the reflecting capacity of silver for light with perpendicular incidence. I should also like to know whether the power used on both instruments was the same; if not, the iris of the observer may have cut off different proportions of the emergent pencil. The light of a star-disk as compared with the illumination of the sky will differ; the higher the power the less the illumination of the sky, area for area. Prof. Pritchard has not told us that the whole of the pencil was used in either case. The whole of the data must be given before we can compare the two instruments.

Mr. De la Rue. I can answer some of those questions. The speculum metal reflector was polished 33 years ago, but it has not suffered during the whole of that time materially. In fact it is as bright now, I dare say, as when I finished it. The With reflector is in as bright a state and as perfect as when it left the maker.

Mr. Ranyard. How long ago was it silvered?

Mr. De la Rue. About three years ago. It is a recent gift to the Observatory, and has been very little used.

Mr. Ranyard. I find a great difference in a few months. Very little sulphur in the air will in a short time make the silver of a yellowish colour.

Mr. Common said that much depended on the place of the eye-pieces, and the power of the wedge to extinguish the light would vary in proportion to the superficies employed of the wedge. The methods employed were not applicable to determine the absolute light.

Mr. Rand Capron said that it would be very desirable that the object-glass and mirror should be of the same focal length.

Prof. Pritchard. They are.

Mr. Rand Capron thought the great desideratum was not to compare the light passing the object-glass with that reflected from the second mirror, so much as to compare it with the light received directly from the first mirror itself without a second reflection.

Mr. Waters. I take it that the determination of the relative light of the stars is an essentially comparative matter, and if carried out with the same instrument as in Prof. Pritchard's method, the result will probably be correct; but it does not follow that his most valuable series of observations will give any data for the comparison of two separate instruments. His observations, which are in such great accord with Prof. Pickering's, were carried out by the reflector entirely, I believe; and the method which he employed then is one essentially adapted to the determination of relative brightness of stars, but not necessarily adapted to determine the relative transmitting power of a reflector and a refractor.

The Astronomer Royal. I feel much interest in this determination of Prof. Pritchard, and the lively discussion we have had shows how generally that interest has been felt; but I think a little more information would be useful. There are some points that affect a comparison between the reflector and refractor—the different focal length, different angles of aperture, different magnifying-powers, and other circumstances—which would not, however, affect the determination of the magnitude of stars made with the same instrument. These latter are relative, and made under similar circumstances; so that although Mr. Common may have criticized some of these points, probably from a misapprehension of the exact circumstances of observation, it does not follow that any doubt is cast upon Prof. Pritchard's results, which, as far as I have examined them, seem to be remarkably accurate. At the same time, in this particular comparison there are certain data required which are not necessary in determining the magnitude of stars. We want to know exactly where the wedge is placed. There seems to be a little uncertainty whether it is at the principal focus or near it. I am not quite clear about it.

Prof. Pritchard. Mr. Common is right in inquiring about that.

The Astronomer Royal. Then I have misunderstood Prof. Pritchard; but we still want a few more details, viz. whether the magnifying-power was the same in the two cases, and what diaphragms were used, and similar details of that sort. There is one point which has not been alluded to. If there are two disks of different sizes, but of the same intrinsic brightness, the larger one will be extinguished less readily than the smaller. The eye is more sensitive to the larger surface, with the same intensity of illumination, than to the smaller one.

Lieut.-Gen. Tennant and *Mr. Bidder* stated what they believed to be the effect of the eyepiece, diaphragms, &c.

Mr. De la Rue said that each telescope had its own eyepiece and magnifying-power, and that the eyepieces were adjusted to the different focal lengths of the two telescopes. In reply to Mr. Ranyard's objection, he thought the silver-on-glass speculum was as bright as when it was put in. It is always covered up, and not exposed in the telescope. The telescopes are always used with the full aperture. I would never admit into my observatory any telescope that required cutting down at all.

A Fellow. Unless the focal length of the two telescopes are equal the angles of incidence of the cone of rays upon the dark wedge are different, consequently the loss of light is different in the two cases.

Prof. Pritchard. The gentlemen have raised these very proper objections: but every one of them has occurred to my own mind, and I think they have all been met. In order to obviate all these difficulties about focal length, all the photometric work is performed with two different telescopes of different apertures and focal length, and then by two different lenses and two different wedges, and then the apertures again are halved, and every thing is tried under varied conditions, in order that we may not be deceiving ourselves. The great object I have endeavoured to secure is this: that the whole of the light that comes to form the images in the common focus in the two lenses should enter into the observer's eye. I have adopted every possible means to secure this, and to the best of my judgment I have secured it.

Mr. De la Rue. All the observations point to this: the desirability of making the determinations with telescopes of the same aperture and of the same focal length.

The President. We must be all exceedingly gratified to have this paper from Prof. Pritchard, not only for its intrinsic importance, but also on account of the interesting discussion it has produced. I feel sure that Prof. Pritchard will clear up any point which may at present be misunderstood, and that you will accord him your thanks for his paper.

Mr. Stone explained a paper on "The proper motions of the 460 stars given in the R.A.S. Memoirs, vol. xxxii., when the places of Auwers's re-reduction of Bradley's observations are adopted instead of Bessel's, with notes of the proper motion of μ Piscium."

The President. Will any of the proper motions, deduced from the old Fundamenta—the large proper motions, for instance—be altered?

Mr. Stone. None of the large proper motions will be altered except for absolute mistakes. In one or two cases Auwers has pointed out mistakes. When we compare the Cape observations of μ Piscium *inter se*, the proper motion in N.P.D. is very small; but the Cape observations compared with Bessel's Fundamenta still give a large value, not much smaller than that given by the Greenwich observations; but Auwers's N.P.D., which is some 13 or 14 seconds different from Bessel's N.P.D., agrees with modern observations without sensible proper motion.

Prof. Pritchard asked whether Mr. Stone had gone through the work of all the observers from the time of Bradley to the present time, or only one or two, because he had found on going over about 40 stars and about 17 of the Pleiades that there was a perplexing variation in the fundamental determinations of the different observatories.

Mr. Stone. No: I have compared the Cape observations among themselves, and taking them *inter se* we get determinations of proper motions independent of those that have been found by comparison of the Greenwich observations with Bradley. On the whole there has been considerable agreement, but some discrepancies. I have not attempted to compare the whole of the recent available material. I know that there are, in the results of different observatories, systematic errors, and I should get different results, though the differences would not be great. If Prof. Pritchard asks whether, on the comparison of the determinations of these proper motions, I have applied the systematic corrections which have recently got much into vogue, then, undoubtedly, I have done nothing of the kind—and deliberately for this reason, that what are called systematic corrections are simply got by comparison of the recent observations at different observatories one with another, and by taking differences simply. I always prefer taking the results of the different observatories and comparing them and seeing how far they agree without corrections of that kind at all, and then by noting the discrepancies you can form some idea whether the results can be trusted.

Mr. Downing. Some years ago I wrote to Prof. Auwers on the subject of proper motions of Bradley's stars, and he told me he intended to publish new proper motions for all Bradley's stars derived from a comparison of the re-reduction of Bradley with the best modern observations. I believe that this work will be of the greatest value when it reaches us; but that was five or six years ago, and Dr. Auwers said then that he hoped to complete it in a few months.

The President was glad that Mr. Stone had brought this subject forward, for any one who had been in the habit of reducing stars from one catalogue to the epoch of another would hail the publication of more accurate proper motions than we have at present. He remembered that many years ago Mr. Stone had been prevented from going into the question of proper motions with regard to one of the Greenwich catalogues simply because it was expected that Prof. Auwers would have published is work on proper motions about that time. That was as far back as 1868 or 1869, but the work has not yet appeared. He trusted it might soon be completed, so that the workers in observatories might be able to have proper motions deduced from the comparison of observations made at Greenwich 130 years ago with those made at the principal observatories in recent years.

The President requested Prof. Adams to give a short account of the results of the Prime Meridian Conference.

Prof. Adams. I have a very short and unvarnished tale to tell you with respect to what we have done. I am only afraid that when I tell you the results we arrived at, you will say, How came you to be so long about it? I can assure you that although the resolutions we arrived at were simple, it took a good deal of discussion to arrive at them. We were a large body of delegates, appointed by different countries; we numbered about forty, representing twenty-four different nationalities. We did not all speak the same language, and that created a little difficulty. We had not only to make speeches, but to get those speeches reported and printed in two languages—English and French. A considerable amount of correction was required before the speeches assumed a form satisfactory to those who uttered them; and several days' interval was required before the next meeting, so that some of us, at one time, rather despaired of coming to any conclusion before Christmas. But finally we got a little more momentum on the machine, and at length succeeded in passing resolutions which met with very general support. The results at which we arrived were perfectly satisfactory to my mind; I think we arrived at the right conclusion on every point.

We were not appointed to bind our respective countries to any thing. The governments that appointed us gave us power to meet and discuss the matter, and, if we could agree, to recommend for adoption to the several countries which we represented, the conclusions at which we had arrived. And that is all we have done. We have not bound our respective countries to any thing whatever, but we hope the different countries will see the desirability of conforming to the resolutions and adopting them. They are very simple. First, that a prime meridian, or initial meridian, should be adopted from which longitudes should be counted, and that that meridian should be the meridian of Greenwich. Secondly, that there should be a universal time-reckoning, founded on the prime meridian of Greenwich; that the time-reckoning should be in mean solar days; and that the origin of the day should be taken at mean midnight on the initial meridian. This would introduce a change of usage of some importance to astronomers, but we cannot and do not pretend to bind astronomers by our resolutions; but we decided that the universal time should conform to the civil reckoning on the initial meridian rather than to the astronomical reckoning: and we recommend that as soon as astronomers and navigators find it practicable, they also should adopt the civil reckoning instead of reckoning the beginning of the day at noon. These resolutions were carried by a considerable majority. There was also a resolution as to the way in which the longitude should be counted. It would, of course, be counted from the first meridian as zero. But there were three different courses open, as there are said to be in politics. The first was that the longitude should be counted in the easterly direction from zero to 360°, so that it would be an even chance whether Greenwich was zero or 360°. The second proposal, which found favour with the Americans, was that longitude should be counted in a westerly direction instead of eastward. They would prefer that America should have a positive longitude, and going round in that way America would come before Europe. The conference at Rome had decided, on their part, that longitude should be counted in an easterly direction from zero to 360°. The Americans did not approve of that. It appeared to several of us that it would be better to start from zero on the initial meridian, and count the longitude both east and west, counting it positive in one direction and negative in the other—east positive and west negative; it did not matter very much which. But, at any rate, in proposing to start in both directions we had the advantage of conforming to the present usage, and therefore it would cause less inconvenience and change. It is equally scientific, at least, to say nothing else, to propose to start from zero and go in both directions, making a break on the opposite side of the globe from positive 180° to negative 180°; and the break would occur at places where it would cause no inconvenience, because there are no inhabitants to speak of in that part of the world; and seamen are already accustomed to change their date at that particular meridian. Altogether it seemed to recommend itself to the majority at the Conference to adopt that plan; and a little diplomacy entered into the subject, because some of the representatives wanted to go easterly and some westerly, and we agreed to both ways, and that settled the matter.

A Fellow. How about the hours?

Prof. Adams. The hours are to be reckoned from zero to 24. We all agreed that there could be no question about that, because it is clear that to reckon by two twelves would be utterly intolerable in any calculation. We begin at zero and go to 24 at midnight, and then change the day. There will be no want of continuity there, because when you get to 24 you drop the hours and change the day.

Mr. Stone thought the Congress had been really successful, and had no doubt that English astronomers would readily accept the change in the way of reckoning time; but he doubted if ordinary people would adopt the new reckoning, and call 1 o'clock in the afternoon 13, and so on, unless an Act of Parliament were procured [No, no!], or unless the railway companies adopted the new time in their time tables.

Prof. Adams. That is the very thing they are going to do. They have already, I believe, agreed to abandon the old time in America. I may mention that this is one of the great recommendations of the change. It is a good deal for railway purposes that the change is required. I have myself gone to meet an evening train, but, on a more accurate investigation of Bradshaw, have found that I ought to have gone in the morning. (Laughter.) That uncertainty ought to be abolished. No doubt a great improvement would be introduced by going to 24 hours; and I believe that in Italy they already do it. I may mention that we do not profess to bind ordinary practice or local practice at all. We passed another resolution, which conciliated some people, to this effect,—that although we introduce this universal time, we do not at all object to the use of local or any other standard time in cases where it might be found convenient. And that was partly intended to meet the case of the American railways. They have introduced a plan in the United States and Canada by which they obtain already a certain degree of uniformity in reckoning the time on the different railways. The railways of the United States extend over several hours difference in longitude, and they found great inconvenience from this circumstance on the different lines; and in order to meet it to a certain extent, without departing altogether from local hours, they introduced a system which is now general. They adopt for railway time the Greenwich time as regards the number of minutes, but they alter the hours. For instance, in those parts of America which are about 5 hours west of Greenwich, they take 5 hours from the Greenwich time; in the parts about 6 hours west they take 6 hours from Greenwich time, and so on. The time therefore differs from Greenwich only by a certain integral number of hours, and there is no occasion to think about any change in minutes. I myself found the convenience of that, for I took my watch showing Greenwich time, and had no occasion to alter it, because when I knew that the railway time differed by so many complete hours, it was easy to know the railway time without any alteration of the watch.

Mr. Stone. The change will be very small as regards the English people, except this change of reckoning up to the 24 hours. We can hardly expect other people to put themselves to considerable inconvenience by introducing changes unless we are prepared to accept this small one, so that if this change of time is to be carried out generally, we ought to begin to make the change here. It will be an inconvenience at first, but I think the change is likely to be a good one; and while we should not attempt to force it on the people, we should try and bring it fully before them. Unless the railways are prepared to carry out the change, I do not think the people would fully enter into it; but if the railways do it, the people will find it more convenient to fall into it than to oppose it.

The Astronomer Royal. This question was brought before the Geodetic Conference at Rome by the Railway and Telegraph Companies, and they insisted on some reform in reckoning time. To reckon from zero to 24 hours would soon become very simple to the public; but it was really for the convenience of the railways and telegraphs that this reform was primarily proposed.

Admiral Sir E. Ommanney. In the Arctic expedition we were supplied with chronometers with 24 hours marked on the dials, and it was very convenient when there was total daylight for a long period, and prevented confusion; and I shall be glad to see the plan come into general use.

Colonel Tupman. Does the Astronomer Royal mean to alter the mean solar day at Greenwich?

The Astronomer Royal. Most distinctly. I propose to adopt it from the 1st of January next year as far as Greenwich is concerned. I think the change will be almost an unmixed advantage to us, because hitherto we have had several different reckonings even in an observatory like Greenwich. I imagine that in many other observatories there is the same difficulty. This change of reckoning will therefore introduce very little inconvenience to astronomers.

Lord Crawford. I would say a word to supplement what Prof. Adams said about Italy. He quoted Italy as being so far advanced as to have already adopted the 24 hours system; but it is the ancient method, an ecclesiastical system, which varied according to the time of sunset.

The President. I am sure we are all obliged to Prof. Adams for his interesting account. I think the public may be easily educated to reckon their time from zero to 24 hours. We have had for a long time at Greenwich, outside the Observatory, and visible to the public, a clock-face marking from zero to 24, and persons who are in the habit of coming up to the Observatory to set their watches are familiar with it.

Lord Crawford. That clock will have to go back 12 hours.

The President. Yes.

Col. Tupman. If a turret-clock is graduated to the 24 hours scale, it will be impossible to distinguish the hour.

The Astronomer Royal. I think with Mr. Stone that we ought to take some public action in this matter by way of advancing this improvement. We might pass some resolution approving of this new reckoning from midnight, and urging its adoption generally by astronomers as well as by railway and telegraph companies. We might also urge on our friends in Ireland to adopt the same reckoning of time. There may be some difficulty in doing that; but it has occurred to me that, following the initiative of the Americans, they might be asked to adopt a new Irish time differing by 1 hour exactly from Greenwich time.

Lord Crawford. It is very much the same in Ireland as in Italy, where there is a particular time in the towns, but Rome's time is kept at the stations.

Mr. Hilger exhibited a new form of solar eyepiece, and explained that he had done away with the second reflection, and thus improved the quality of the definition, at the same time that the light reflected to the eye was diminished by one half.

Mr. Ranyard. The chief objection is that the light necessarily goes through a great thickness of glass which is unequally heated, and unless the glass is very good optically, the definition is not so good for high powers as when it is reflected from the front surface, as in the case of the Herschelian prism.

The following papers were announced and partly read:—

G. Davidson. "Occultations of Stars by the Moon, observed at the Davidson Observatory, San Francisco."

Communicated by *Mr. Gill.* "Observations of Comet 1884 (Barnard) made at the Royal Observatory, Cape of Good Hope."

W. H. Finlay. "Approximate Elements of Comet 1884 (Barnard)."

Rev. T. E. Espin. "A remarkable Configuration of Stars in the Milky Way, detected by photography."

E. B. Powell. "On the Periodic Time of α Centauri."

Prof. Pritchard. "Note on a Comparison of the Photometric Magnitudes of the same Stars observed at Harvard College and at the University Observatory, Oxford."

Communicated by *Prof. Pritchard.* "Observations of Stars occulted by the Moon during the Eclipse of 1884, Oct. 4."

Communicated by *Mr. Stone.* "Total Eclipse of the Moon, 1884, Oct. 4."

Dr. R. Copeland. "Occultations of Stars observed at Dun Echt during the total Lunar Eclipse of Oct. 4, 1884."

Rev. S. J. Perry. "Total Eclipse of the Moon Oct. 4, 1884."

A. A. Common. "Note on a Method of giving long exposures in Astronomical Photography."

A. A. Common. "Note on Stellar Photography."

Dr. J. Morrison. "The Orbit of Barnard's Comet, 1884."

Lt.-Col. Tupman. "Occultations observed at Harrow during the Total Eclipse of the Moon, 1884, Oct. 4."

C. Gogou. "Sur une inégalité lunaire à longue période."

Rev. S. J. Johnson. "Abnormal Obscurity of the Moon in the late Eclipse."

W. F. Denning. "The Total Eclipse of the Moon, 1884, Oct. 4."

J. Tebbutt. "Observations of Comet Barnard, 1884."

The Ballot was then proceeded with, and the following Astronomers were duly elected Foreign Associates of the Society:—Prof. Th. Bredichin, Moscow Observatory; Prof. Edward S. Holden, Washburn Observatory, Madison, Wisconsin; M. Magnus Nyrén, Pulkowa Observatory.

M. Pietro Baracchi was duly elected a Fellow of the Society.

The Meeting adjourned at 10^h P.M.

Meeting of the Liverpool Astronomical Society.

The usual monthly Meeting was held at the Association Hall on the 10th of November, the President, the Rev. T. E. Espin, B.A., in the chair. The minutes of the previous meeting having been read and approved, the Secretary, Mr. W. H. Davies, said he should like to take the opinion of the meeting on a question of some importance. Their continued success, paradoxical as it might seem, had led them into some fianancial difficulty. The number of their members had increased very rapidly, but it had not kept pace with the improvement of their publications. It would seem an easy remedy to either reduce the publications or to raise the subscriptions; but the Council were very reluctant to do either, as many of the papers were of such value that the printing expenses were small by comparison, whilst, to raise the subscription would be to take the Society out of the reach of the class of amateurs it was intended to encourage. They had begun by printing an 8-page pamphlet of the Abstracts of their proceedings, and this had increased in size, until last month the matter filled 28 pages, besides woodcuts and lithographs. Of course this could not be continued without a certain loss, and it had been very seriously considered whether it would not be well to issue their proceedings in the form of a "Journal," and reserve a portion of the cover for such scientific advertisements as were likely to meet the requirements of their members. It had been feared that such an innovation would lessen the dignity of a scientific society; but in his eyes it was more dignified to utilize all their resources than to stand hat in hand and depend upon donations.

The President said the course proposed was a bold one; but as the profits would be all placed to the publishing fund, every member would receive a direct benefit and he could see no objection to the plan. Mr. R. E. Johnson, F.R.A.S., and the Rev. J. Bone (Lancaster) supported the motion, which, after some discussion, was carried unanimously.

The President then called the attention of members to a valuable present which had that day been received from Mr. R. S. Newall, F.R.S. It consisted of an enlarged copy of Lassell's nebula in Orion, and as it contained all Herschel's and Lassell's stars ready numbered for reference, he was sure it would be exceedingly useful.

A paper on "Spectroscopes to small Telescopes," by Mr. T. R. Clapham, was read. He had always been ambitious to see the solar prominences with his $3\frac{1}{4}$-in. Wray, but, from what he had gathered from other workers, nothing less than 8 prisms to a 6-in. O.G. would show them. However, he applied one of Browning's 5-prism direct-vision spectroscopes by simply screwing it to a slip, instead of a tube, connected to the telescope, and by this simple contrivance he was enabled to see the F line reversed and, shortly after, vary in position. He watched this for nearly half an hour, and during this time it widened to a cloud-like head and then gradually subsided.

A paper by Mr. J. Baxendell, F.R.S., F.R.A.S., was read on the new variable star V Boötis. The star is near DM. No. 2642+16°. Its range of variation is from 9·2 to 10·2 magn.; it has a dull orange colour, which, however, is subject to slight variation, and its image is generally remarkably sharp and distinct. The mean period, derived from seven minima, is 122 days and epoch=Dec. 8·2, 1881. Observations of four maxima give a mean period of 120·8 days and epoch=March 2·7, 1882. The greatest difference between the calculated and observed times of minima is 10·2 days, and the mean difference 5·4 days. The corresponding differences for the four maxima are only 2·2 and 1·4 days.

In a paper on the variable star R Ursæ Majoris, Mr. J. Baxendell, junior, said:—From a series of observations which he had made since the 26th of May last, he found that a maximum occurred on the 31st of August, or 12 days earlier than the time given in the ephemeris in the September number of the 'Observatory;' but, from the results of observations made by his father in the years 1857 to 1859 and 1861 and 1862, it appeared that a similar difference occurred in 1859 and that in 1862 the difference was a little over 11 days. Professor Schönfeld and others had also observed similar irregularities.

Mr. W. F. Denning, F.R.A.S., stated, in a papar on a shower of meteors from α Aurigæ, that on September 22, 1884, he had watched the eastern sky for $3\frac{1}{4}$ hours during the period from $10^h\ 40^m$ to $14^h\ 40^m$ and observed 36 shooting-stars, of which 8 were directed from the point $\alpha\ 73°\ \delta+45$, slightly W. of α Aurigæ. The radiant was very sharply defined and furnished meteors of the swift streak-bearing class. He had seen this stream with equal distinctness on September 21, 1879. On that night he counted 92 shooting-stars in a persistent watch of $5\frac{1}{2}$ hours, from 11^h to $16^h\ 30^m$, and registered 8 paths from a centre at $\alpha\ 76°\ \delta+44°$; they nearly all left streaks and were described as swift. Evidently therefore this epoch of September 21–22 was a special one as regarded this radiation of meteors from the immediate region of Capella, and the shower well deserved re-observation from its richness and the definite character of the point of its divergence.

In a carefully prepared and beautifully illustrated paper on Lunar surfacing, Mr. S. E. Peal advocated a theory as to the origin of the circular formations based on the assumption that the entire phenomenon had been an exceedingly slow, quiet, and cold one, the result of expiring energies and the reverse of heated, violent, or explosive. It was admitted by astronomers that the Earth, Venus, and Mars formed a series as regarded temperature, air, and moisture, and the Moon seemed to follow next. With its exceedingly rare atmosphere, he took it that the lunar seas would be frozen over while yet the internal temperature was proportionately high. The walled plains he took to be lagoons or lakes, the water having been slowly raised as moisture and deposited as a rampart around the liquid area. The larger circles had been regarded as seas, whilst the smaller were ascribed to volcanic action; but he claimed that they had all one origin, and that the origin admittedly possible for the seas was equally so for the walled plains, craters, and craterlets.

Note on the Photometer employed at the Oxford Observatory in the Comparison of Refracting and Reflecting Telescopes.

Some considerable misapprehension of the real construction of the photometer used in the above investigations was manifested during the discussion at the last meeting of the Royal Astronomical Society; and this I am anxious to remove. Before I regarded the photometer as scientifically applicable to the relative measurement of the lustre of the stars, I took especial care that the construction of the instrument should be such, that all the light proceeding from the star on to the object-glass or mirror must pass directly through the photometer-wedge on to the retina of the observer. As measured by a *dynameter*, this light occupies on the eye-lens of the photometer a small circle of one thirtieth of an inch in the case of the refractor, and one twentieth of an inch in the case of the mirrors. Close to this luminous circle, which becomes the base of a luminous cylinder when it leaves the lens, slides the photometer-wedge, and close behind this is the observer's eye, guided and restricted by a small diaphragm pierced with a circular hole of one tenth of an inch in diameter. The position also of the star itself is secured in the axis of the telescope by means of the pierced diaphragm of a *Dawes's solar eyepiece*. I maintain, therefore, that it is not easy to devise, or even to conceive, a photometer practically more simple or accurate than the one in question.

Any person taking an interest in the subject, if he will take the trouble of comparing the results in Vol. xi. of the 'Annals of Harvard Observatory' with those in Vol. xlvii. of the 'Memoirs R.A.S.,' will find that the deviations of the individual measures from the adopted mean measure of the star's magnitude are, in the case of the American instrument, fully double those resulting from the English photometer; and this statement is in nowise inconsistent with admitting the excellence of the American final results.

About 1800 stars have now been measured at both observatories, of which about 1570 differ by a quantity not exceeding one quarter of a magnitude, and out of these latter 717 agree to the tenth of a magnitude. Results such as these appear sufficient to establish the scientific value of both instruments. C. PRITCHARD.

University Observatory, Oxford,
1884, Nov. 20.

Pending Problems of Astronomy.

[Continued from p. 326.]

METEORS and the comets, seeming to belong neither to the solar system nor to the stellar universe, present a crowd of problems as difficult as they are interesting. Much has undoubtedly been gained during the last few decades, but in some respects that which has been learned has only deepened the mystery.

The problem of the origin of comets has been supposed to be solved to a certain extent by the researches of Schiaparelli, Heis, Professor Newton, and others, who consider them to be strangers coming in from outer space, sometimes "captured" by planets, and forced into elliptic orbits, so as to become periodic in their motion. Certainly this theory has strong supports and great authority, and probably it meets the conditions better than any other yet proposed. But the objections are really great, if not insuperable,—the fact that we have so few, if any, comets moving in hyperbolic orbits, as comets *met* by the Sun would be expected to move; that there seems to be so little relation between the direction of the major axes of cometary orbits, and the direction of the solar motion in space; and especially the fact, pointed out and insisted upon by Mr. Proctor in a recent article, that the alteration of a comet's natural parabolic orbit to the observed elliptic one, by planetary action, implies a reduction of the comet's velocity greater than can be reasonably explained. If, for instance, Brorsen's comet (which has a mean distance from the Sun a little more than three times that of the Earth) was really once a parabolic comet, and was diverted into its present path by the attraction of Jupiter, as generally admitted, it must have had its velocity reduced from about eleven miles a second to five. Now it is very difficult, if not out of the question, to imagine any possible configuration of the two bodies and their orbits which could result in so great a change. While I am by no means prepared to indorse as conclusive all the reasoning in the article referred to, and should be very far from ready to accept the author's alternative theory (that the periodic comets have been ejected from the planets, and so are not their captives, but their children), I still feel that the difficulty urged against the received theory is very real, and not to be evaded, though it may possibly be overcome by future research.

Still more problematical is the constitution of these strange objects of such enormous volume and inconceivable tenuity, self-luminous and transparent, yet reflecting light, the seat of forces and phenomena unparalleled in all our other experience. Hardly a topic relating to their appearance and behaviour can be named which does not contain an unsolved problem. The varying intensity, polarization, and spectroscopic character of their light; the configurations of the nucleus and its surrounding nebulosity; and especially the phenomena of jets, envelopes, and tail,—all demand careful observation and thorough discussion.

I think it may be regarded as certain, that the explanation of these phenomena when finally reached, if that time ever comes, will carry with it, and be based upon, an enormous increase in our knowledge as to the condition, contents, and temperature of inter-planetary space, and the behaviour of matter when reduced to lowest terms of density and temperature.

Time forbids any adequate discussion of the numerous problems of stellar astronomy. Our work, in its very nature incessant and interminable, consists, of course, in the continual observation and cataloguing of the places of the stars, with ever-increasing precision. These star-places form the scaffold and framework of all other astronomical investigations involving the motions of the heavenly bodies: they are the reference-points and bench-marks of the universe. Ultimately, too, the comparison of catalogues of different dates will reveal the paths and motions of all the members of the starry host, and bring out the great orbit of the Sun and his attendant planets.

Meanwhile, micrometric observations are in order, upon the individual stars in different clusters, to ascertain the motions which occur in such a case; and the mathematician is called upon again to solve the problem of such movement.

Now, too, since the recent work of Gill and Elkin in South Africa, and of Struve, Hall, and others elsewhere, upon stellar parallax, new hopes arise that we may soon come to some wider knowledge of the subject; that, instead of a dozen or so parallaxes of doubtful precision, we may get a hundred or more relating to stars of widely different brightness and motion, and so be enabled to reach some trustworthy generalizations as to the constitution and dimensions of the stellar universe, and the actual rates of stellar and solar motion in space.

Most interesting, also, are the studies now so vigorously prosecuted by Professor Pickering in this country, and many others elsewhere, upon the brightness of the stars, and the continual variations in this brightness. Since 1875, stellar photometry has become almost a new science.

Then, there are more than a myriad of double and multiple stars to watch, and their orbits to be determined; and the nebulæ claim keen attention, since some of them appear to be changing in form and brightness, and are likely to reveal to us some wonderful secrets in the embryology of worlds.

Each star also presents a subject for spectroscopic study; for although, for the most part, the stars may be grouped into a very few classes from the spectroscopic point of view, yet, in detail, the spectra of objects belonging to the same group differ considerably and significantly, almost as much as human faces do.

For such investigations, new instruments are needed, of unexampled powers and accuracy, some for angular measurement, some for mere power of seeing. Photography comes continually more and more to the front; and the idea sometimes suggests itself, that by and by the human eye will hardly be trusted any longer for observations of precision, but will be superseded by an honest, unprejudiced, and unimaginative plate and camera. The time is not yet, however, most certainly. Indeed, it can never come at all, as relates to certain observations; since the human eye and mind together integrate, so to speak, the impressions of many separate and selected moments into one general view, while the camera can only give a brutal copy of an unselected state of things, with all its atmospheric and other imperfections.

New methods are also needed, I think (they are unquestionably possible), for freeing time-observations from the errors of personal equation; and increased precision is demanded, and is being progressively attained, in the prevention, or elimination, of instrumental errors, due to differences of temperature, to mechanical strains, and to inaccuracies of construction. Astronomers are now coming to the investigation of quantities so minute, that they would be completely masked by errors of observation that formerly were usual and tolerable. The science has reached a stage, where, as was indicated at the beginning of this address, it has to confront and deal with the possible unsteadiness of the Earth's rotation, and the instability of its axis. The astronomer has now to reverse the old maxim of the courts: for him, and most emphatically at present, *de minimis curat lex.* Residuals and minute discrepancies are the seeds of future knowledge, and the very foundations of new laws.

And now, in closing this hurried and inadequate, but I fear rather tedious, review of the chief problems that are at present occupying the astronomer, what answer can we give to him who insists, *Cui bono*? and requires a reason for the enthusiasm that makes the votaries of our science so ardent and tireless in its pursuit? Evidently very few of the questions which have been presented have much to do directly with the material welfare of the human race. It may possibly turn out, perhaps, that the investigation of the solar radiation, and the behaviour of sun-spots, may lead to some better understanding of terrestrial meteorology, and so aid agricultural operations and navigation. I do not say it will be so,—in fact, I hardly expect it,—but I am not sure it will not. Possibly, too, some few other astronomical investigations may facilitate the determination of latitudes and longitudes, and so help exploration and commerce; but, with a few exceptions, it must be admitted that modern astronomical investigations have not the slightest immediate commercial value.

Now, I am not one of those who despise a scientific truth or principle because it admits of an available application to the affairs of what is called "practical life," and so is worth something to the community in dollars and cents: its commercial value is—just what it is—to be accepted gratefully.

Indirectly, however, almost all scientific truth has real commercial value, because "knowledge is power," and because (I quote it not irreverently) "the truth shall make you free,"—any truth, and to some extent; that is to say, the intelligent and intellectually cultivated will generally obtain a more comfortable livelihood, and do it more easily, than the stupid and the ignorant. Intelligence and brains are most powerful allies of strength and hands in the struggle for existence; and so, on purely economical grounds, all kinds of science are worthy of cultivation.

But I should be ashamed to rest on this lower ground: the highest value of scientific truth is not economic, but different and more noble; and, to a certain and great degree, its truest worth is more as an object of pursuit than of possession. The "practical life"—the eating and the drinking, the clothing and the sheltering—comes *first*, of course, and is the necessary foundation of any thing higher; but it is not the whole or the best or the most of life. Apart from all spiritual and religious considerations, which lie outside of our relations in this association, there can be no need, before this audience, to plead the higher rank of the intellectual, æsthetic, and moral life above the material, or to argue that the pabulum of the mind is worth as much as food for the body. Now, it is unquestionable, that, in the investigation and discovery of the secrets and mysteries of the heavens, the human intellect finds most invigorating exercise, and most nourishing and growth-making aliment. What other scientific facts and conceptions are more effective in producing a modest, sober, truthful, and ennobling estimate of man's just place in nature, both of his puny insignificance, regarded as a physical object, and his towering spirit, in some sense comprehending the universe itself, and so akin to the divine?

A nation oppressed by poverty, and near to starving, needs first, most certainly, the trades and occupations that will feed and clothe it. When bodily comfort has been achieved, then higher needs and wants appear; and then science, for truth's own sake, comes to be loved and honoured along with poetry and art, leading men into a larger, higher, and nobler life.

C. A. Young.

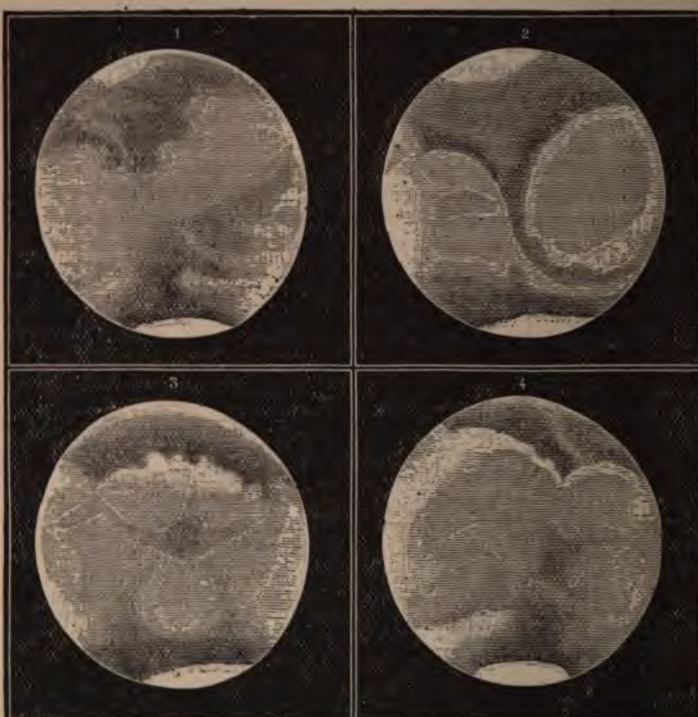
The Planet Mars in 1884.

M. TROUVELOT, who has devoted much attention to this planet during the late opposition,—a specially important one since the comparatively unknown northern hemisphere was presented to us,—has published four of the numerous sketches which he has made, in the September number of 'L'Astronomie.' The four have been selected, since they give a view of nearly the whole of the northern hemisphere of Mars. M. Trouvelot adds the following descriptive notes, Mr. Green's nomenclature being used throughout:—

"Fig. 1, Mar. 16, $7^h\ 20^m$. To the south west, the east extremity of Herschel II. Strait (Kaiser Gulf) is seen terminating near the Forked Bay (the bay of the prime meridian). On the south-east, De la Rue Ocean (Kepler Ocean) is distinguishable, approaching almost to the terminator. Burton Bay forms the extreme north point, situated a little to the west of the central meridian. Between the solid mass which abuts on Burton Bay and that joining the Forked Bay is a narrow whitish band joining Beer Continent (Copernicus Continent) to Phillips Island. A white spot, doubtless caused by vapours, is seen to the south-west and near the edge of these great dark spots. The north-polar spot diminishes in size, above it; towards the south is Campani Sea (Faye Sea) and Knobel Sea, which appears very dark, and is sharply detached from Rosse Land, which is, however, less brilliant than usual this evening. Knobel Sea curves back a little towards the east, towards Tycho Sea, and is separated from it by a somewhat broad, but at the same time very indistinct, whitish band. Tycho Sea (Lacaille Sea) forms, at first sight, a dark quadrilateral, which towards the top is surmounted by an angular spot of lighter tint, separated from the quadrilateral by a whitish band. On the east this quadrilateral is to a great extent separated by a whitish band from a grey spot which joins the terminator and belongs to Airy Sea. To the north-west, on the limb, the extremities of Lassell Sea and Leverrier Land are visible.

"Fig 2, Feb. 15, $6^h\ 45^m$. Kaiser Sea (the Hourglass Sea) just crosses the central meridian. As usual, it is much darker, nearly black, on the eastern side, which is bordered by an irregular very bright fringe. Towards the top the bright fringe runs into Tycho and forms Cape Banks, which projects a good way into the interior. Flammarion Sea on the west is also fringed with white, also Hooke Sea, which is above it. Flammarion Sea is divided from Kaiser Sea on the east by a narrow isthmus, which enlarges on the south and forms a whitish triangle in the midst of the latter sea. The bay forming Main Sea is visible, but very indistinctly. Towards the lower or northern extremity of Kaiser Sea, where it is very narrow, and where by a swelling at the east it generates Nasmyth Inlet, it would seem that this narrow sea is separated from the rest by a small white band; this must be caused by vapours or clouds traversing the strait, for I have never noticed this break before. The north-polar spot is bordered by Delambre Sea, which, towards the west, is strongly marked, and rises towards the south, where it diminishes angularly near Main Sea. Laplace Land seems to communicate directly with the great Herschel I. Continent by a narrow whitish isthmus. Between the south-west extremity of Main Sea and Huggins Bay a somewhat bright white spot is seen.

"Fig. 3, Feb. 27, 7^h 45^m. On the south there is seen near the limb that part of Maraldi Sea which extends from Burchardt Land to beyond Trouvelot Bay. The north edge of this long sea is fringed by a luminous band which follows its numerous windings. Slightly to the west of the middle of the immense arc formed by this spot Noble Cape is clearly seen, forming an indentation of



dazzling whiteness. Not far from the centre of the disk a very singular grey oval spot is visible, very diffused at the edges, which on the east and west is connected with Huggins and Trouvelot Bays by a narrow and indistinct greyish band, which bends to rise again towards them. This singular oval spot *was certainly not visible in*

1877, 1878, *and* 1879, *when Mars was much nearer us* *. This oval spot is also connected with Maraldi by another narrow greyish band which runs north and south, and which I have often observed before. From the edges of the north-polar spot two angular spots are seen projecting towards the south.

"The more easterly one runs towards the oval spot in bending towards the west, and becomes effaced a little before reaching it. The westerly forms a very decided curve, and returning again towards the east, gradually fades away and is united to the oval spot by a scarcely perceptible band.

"A bright white spot, curved and stretching almost to the limb, is visible to the west of this spot.

"Fig. 4, Mar. 2, $6^h\ 40^m$. To the south, the western part of Maraldi Sea is visible, Trouvelot Bay forming an angle a little to the east of the central meridian. On the south-west, quite close to the limb, is the large narrow spot, which comes from the south and ends under Terby Sea. A faint greyish spot, already described, runs from Trouvelot Bay, enlarging and curving towards the west. This spot joins Maraldi by a narrow faint greyish band, a little to the west of Trouvelot Bay. The north polar spot is surrounded on the south by a large dark spot (doubtless Oudemann Sea), which turns upward towards the south, and is soon divided by a narrow whitish band. Continuing from thence, but less distinctly, it forms an angular spot with edges very diffused and difficult to trace. To the east of Oudemann Sea, near the limb, Fontana Land is visible, though not very bright. To the west of this sea, and a little above the polar spot, is an elongated white spot very easily visible, which is bright near Oudemann Sea, and becomes fainter as it approaches the limb, with which it becomes confused. The point where the terminator meets the south edge of the planet is *clearly indented*, for the curve instead of being elliptical, as it should be were this part of the surface perfectly spherical at this point, forms a very distinct obtuse angle, which indicates a *considerable elevation* of the surface at that point. This part of the edge appears also more luminous than the other parts."

M. Trouvelot points out in addition that the dark spots are far larger, more numerous, and better defined in the southern than in the northern hemisphere of Mars, and that certain spots are variable in shape and tint. "As yet we have not sufficient facts to decide with certainty as to the cause of these changes, whether they result from an effect of illumination or whether they are rather produced by the variations of seasons, by rains, mists, or clouds."

* [The oval spot is the Oceanus Fluvius of the 1877 map of Schiaparelli, and the three canals connecting it with Maraldi Sea are his Titanum, Lœstrygonum, and Cyclopum. They were observed by Schiaparelli on several occasions in 1877. The Oceanus Fluvius was distinctly seen in 1877 at Greenwich, but the canals were not detected. The Cyclopum was seen on 1879, Nov. 11 and 12, and the entire district was figured on 1882, Jan. 9, almost precisely as M. Trouvelot has depicted it. It was likewise seen under much the same aspect during the 1877 opposition at Potsdam, and during 1879 by the late Mr. C. E. Burton.—EDITOR.]

Neptune.

is remarkable that Dr. G. Müller stopped his observations of eptune on 1883 November 27, the night I commenced, and that rof. Pickering commenced his observations on Dec. 15, the night stopped!

It has been already noticed that on the latter night the estimates agreed within a tenth of a magnitude, mine being the larger numerically; and similarly on the former night the estimates agree within half a tenth, mine being again the larger. The observations on Nov. 27 were not simultaneous as on Dec. 15; but as the *large* variations of light did not commence until Nov. 29, a comparison is possible.

With reference to Dr. Müller's communication to the 'Astronomische Nachrichten,' and to the comparison of his observations with my epoch and period, perhaps it should have been stated more distinctly that the epoch could only be used for the interval between Nov. 29 and Dec. 14, 1883; for supposing the variation to be caused by a darkening of a part of the surface of the planet, we have no right to assume that the process always occurs at the same place or on the same meridian; and I was not surprised to see that there was no apparent connection among his observations thus treated. But referring to the 'Observatory,' No. 89, p. 264, let us omit his last observation, Dec. 30, when all variation had ceased; and let us also omit his first observation on Oct. 7, when either some strongly marked variation occurred, or when the observation was influenced by some disturbing cause. We shall then be left with five observations between Oct. 13 and Nov. 27, showing some very minute variations.

Now by making some trifling adjustments to epochs of max. and min., these minute variations show a period of 7·92 hours.

Potsdam M.T.	Obs. magn. Neptune.	Adopted Epochs of M and m.	Intervals.
1883, Oct. 13d 8h·5	8·08	Oct. 13d 7h·0 m.	673h·2
Nov. 10 6·6	8·08	Nov. 10 8·2 m.	194·0
18 9·8	7·95	18 10·2 M.	142·6
24 7·1	7·97	24 8·8 M.	71·3
27 9·1	7·95	27 8·1 M.	

and these intervals correspond to 85, 24½, 18, and 9 periods of 7·92 hours each.

But we must not trust to such minute variations, but wait unt large variations again occur.

Neptune will thus, in the course of time, become the means establishing uniformity in estimating magnitudes among telesco stars; perhaps Prof. Pickering would kindly make some sug tions for general use during the approaching opposition of planet.

MAXWELL HA

Kempshot Observatory, Jamaica,
1884, Sept. 25.

Results of Observations of the Variable Star S Coronæ.

While engaged some months ago in reducing and discussing my observations of this variable, George Knott, Esq., F.R.A.S., kindly communicated to me the results of his observations, to be used, in connection with my own, for a redetermination of the star's elements. I have therefore combined the two series in the following Table, the first column of which contains the initials of the observers, the second the observed dates of maximum, the third the observed magnitude, the fourth the number of days between these maxima, and the fifth the number of included periods. The tabulated dates of the maxima marked B.K. are the mean dates of the two observers.

Observers' Initials.	Observed dates of maximum.	Observed magnitude.	Interval in days.	No. of Periods.
B.	1864, Aug. 11	6·7		
B.	1865, July 17	6·6	340	1
B.K.	1866, July 27	7·8	375	1
B.K.	1867, Aug. 13	6·6	382	1
B.K.	1868, July 21	6·7	343	1
K.	1869, July 16	7·2	360	1
K.	1870, July 8	6·5	357	1
K.	1871, July 4	6·8	361	1
K.	1872, July 21	7·4	383	1
K.	1877, June 3	7·0	1778	5
B.K.	1878, May 27	6·8	358	1
B.	1880, May 26	7·2	730	2
B.	1881, May 30	6·4	369	1
B.	1882, May 9	7·4	344	1
B.	1884, April 30	6·6	722	2

Treated in the usual way these data give

Mean Period = 360·416 days, Epoch = 1873, June 27·0.

Comparing the dates of maxima calculated from these elements with the observed dates, we have the following differences (C – O):—

− 1·78	+ 0·32	+ 7·06
+ 18·64	+ 3·74	− 2·10
+ 4·06	+ 3·16	− 10·68
− 17·52	− 19·46	+ 5·74
− 0·10	+ 4·64	+ 4·58

It will be seen from these numbers that the period is subject to considerable irregularities, which occasionally produce deviations from the calculated times of maximum amounting to about one nineteenth of the whole period.

› minima have for many years past occurred at or ...
when the star was too near the Sun for satisfactory obser-
n; but from one well-observed minimum in 1866, and two
rfectly observed in 1867 and 1869, it appears that the mean
rval from minimum to maximum is about 126 days, and from
imum to minimum about 234 days.

JOSEPH BAXENDELL.

The Observatory, Birkdale, Southport,
1884, Nov. 3.

A Galvanic Contact Apparatus for Chronometers *.

So far as we were aware when we were requested by the Queensland Government to construct some chronometers fitted with galvanic contact apparatus, such apparatus had never been applied in a satisfactory way to chronometers before; and the matter offered some difficulties in consequence of the small power available, and the absolute necessity of doing the work without interfering with the rate of the instruments.

The system adopted by us for the contact work of astronomical clocks seemed to be that best adapted for chronometers. This consists of a pair of very light insulated springs, one of which is in electrical connection with the battery and the other with the line-wire. The springs are so placed in the clock, that one of them is lifted every second until it makes contact with the other, when the current passes. Immediately afterwards it returns to its normal position, and the current ceases until the succeeding second, when the operation is repeated.

The escapement is modified so as to allow of the work being done after the pendulum has received its impulse, and therefore at a time when it is entirely disconnected with the clock-train.

By this arrangement the going of the clock is unaffected by any variation in friction produced by changes in the adjustment of the springs. The springs are mounted on a carriage fitted with cross-slides, so that they can be adjusted to their proper positions with the greatest accuracy.

A fuller description of this contact work will be found in 'Nature,' Vol. xx. No. 510.

Apparatus of this description is in use at the Royal Observatory, Greenwich, and at most observatories, public and private, where a chronograph is employed.

In order to allow of the same system being applied to a chronometer, certain alterations in the escapement and train became necessary. In a clock with a seconds pendulum, the motion of

* [Having seen at the Royal Observatory, Greenwich, a chronometer ordered for the Queensland Government, having a galvanic contact apparatus attached for the purpose of marking seconds on a portable chronograph to be used in a survey, we thought a description of it might be interesting, as such an arrange-ment would prove useful to many amateur astronomers. Messrs. E. Dent & C... therefore kindly prepared for us the above account.—EDITOR.]

the escape-wheel is slow, and the required conditions may be fulfilled by causing the springs to be lifted by a wheel mounted on the axis of the escape-wheel (which also carries the seconds hand). But a chronometer beats half-seconds, and therefore the angular motion of the axis of the hand at each beat is only one half what it is in a clock: and the wheel must be considerably smaller in diameter. It seemed, therefore, that the best way would be to make contact by a wheel mounted on the same axis as the escape-wheel, which in this instance (owing to the modifications in the escapement necessitated by the condition that the contact should not affect the rate) made a revolution in 6 seconds only, that is, in $\frac{1}{10}$ of the time of the seconds or fourth wheel as it is called. But here the motion was much more rapid, and there was probably not more than $\frac{1}{20}$ of the power available, owing to the friction of the additional wheel and pinion; it became evident, therefore, that the action of the springs must be exceedingly light. To lessen the power required, it was decided to do with one spring only, and to cause the chronometer to break contact instead of to make it. This had a twofold advantage, as besides there being only one spring to lift instead of (for an instant only) two, the time during which the current passed was considerably lengthened.

With a "make" instead of a "break" of the contact, the current would have passed during such an extremely small space of time, that it is probable the electro-magnet on the chronograph would not have down its work with certainty; but a "break," however short in duration, is much more effective. Therefore the apparatus employed in these chronometers differed from that used for clocks, inasmuch as the one spring, when at rest in its normal position, reposed on a little stud or pillar in connection with the battery-wire, and the current flowed through. But when lifted by the wheel-tooth each second the circuit was broken and the current ceased, until the spring, having been dropped by the wheel, resumed its former position.

The escapement was modified by increasing the angular motion of the escape-wheel, and so arranging matters so as to allow the impulse to be given to the balance before the spring was acted on, and to confine the action on the spring to the period during which the balance was entirely free from any connection with the train.

This arrangement appears to answer extremely well; indeed, so far as our present experience goes, it leaves nothing to be desired. The springs are mounted on a carriage, provided with slides for adjustment, and removable from the chronometer with facility. Electrical connection is made to binding-screws outside the brass box of the chronometers, and from these to others outside the wood box, to which the battery and line-wires are to be attached. The chronometers were not mounted in gymbals, but were fitted into padded mahogany boxes, so as to be more portable.

E. Dent & Co.

61 Strand, London, W.C.,
1884, Nov. 10.

Photographing the Solar Corona.

THE results obtained in the 1882 Eclipse Expedition showed that the light of the solar corona is very rich in blue and violet rays; and it was this discovery that led Dr. Huggins to attempt to photograph the solar corona without an eclipse. That such a feat was possible, and might eventually be accomplished, had been expected ever since it was found possible to observe the prominences on any fine day; and if the coronal spectrum had consisted of a few bright lines, as the spectrum of the prominences does, it would have been accomplished long since. It was obvious, however, that since the spectrum of the coronal light was mainly continuous, the greater the dispersion used, the less likely was the corona to be separated from the atmospheric glare that overpowered it. In the first attempts to see the prominences without an eclipse, coloured glasses were made use of without success; but this method seemed the only one available for the corona, and it was in this direction that Dr. Huggins experimented as soon as the photographic record of the coronal spectrum indicated what part of the spectrum was likely to give the greatest chance. Blue glass was first used and then a solution of permanganate of potash, with results that appeared to be successful. But the use of any such media was decidedly objectionable, as they were likely to induce a scattering of rays from the image of the Sun itself: this objection Dr. Huggins was himself the first to acknowledge; and he argued, since photographic films are themselves only slightly acted upon by any but the blue and violet rays, where is the necessity for an absorbing medium at all? He soon found that he could get upon his plate the corona, or, to speak less decisively, a coronal appearance round the Sun, without the blue glass or permanganate; and when I was sent out to Switzerland to put the process to a practical test, I was not even provided with them, for so long as they were used there could be little certainty of the character of the results, even if the absorbing medium was not really inimical to the process.

The [illegible] of the method rests upon the process familiar to photographers as "heightening contrast." Three degrees of light and shade have to be taken into consideration:—

1. The light of the Sun.
2. The light of the corona reinforced by atmospheric glare.
3. Atmospheric glare.

The second and third, differing in intensity so slightly that the eye is incapable of separating them, have, by means of the power in the photographer's hand, to be [illegible]; but before this can be done, two physical effects, the cause of which lies primarily in the intense brilliancy of the Sun, have to be eliminated. These are:

The effect produced by reflections from the back of the plate—known as "halation."

The spreading of the photographic action in a lateral direction.

This latter defect might practically be disregarded. Though photographers are aware that development extends not only downwards into the film, but also around the particles acted upon by light, this does not take place to a sufficient extent to induce a corona-like appearance round the limb of a greatly overexposed photograph of the Sun, or it would also be present in pictorial photography to such an extent as to greatly mar the definition of the "high lights." Photographers who have had any experience in taking interiors, know that their only enemy is "halation."

"Halation" Dr. Huggins overcame by the well-known method of backing his plates with asphaltum dissolved in benzene or turpentine, a compound bearing the more familiar but less æsthetic name of "Brunswick black." Dr. Huggins's corona photos were objected to by some on the ground that unevenness of backing might have something to do with their appearance. Such an objection, however, could only have been put forward by those whose experience of halation marks was limited, and who had never tried what the result would be by taking a photograph of the Sun under such conditions. Happily, one of the results of my Swiss expedition has been to remove this objection entirely.

One other method was proposed for doing away with both "halation" and any possible "lateral development." This was to block out the Sun's image by using a disk. This method was not adopted by Dr. Huggins; but was not only tried by myself in Switzerland, but put into daily use—with what advantages one month's observations testify.

The first difficulties disposed of, the separation of the corona from the atmospheric glare had to be effected; and though the difference between the brightness of the glare and that of the corona and glare together would be very slight, Dr. Huggins showed that it could be done, and in a very similar manner to that by which two pieces of paper so near alike in whiteness as to appear identical, may by a series of photographic operations be represented side by side, the one white and the other black. The sky can be impressed upon a plate in a small fraction of a second. To get the corona reinforced as it is by the atmospheric glare, the exposure must be a little less than would be necessary for the atmospheric glare alone. A well-restrained developer gives the brighter parts of an image a further advantage over the other, and very slow development should enable the action to be stopped at any moment. Intensification after development heightens contrast still further.

I should not have ventured to give the foregoing explanation at such length in this paper, but for a prevailing idea amongst astronomers that photography is so purely mechanical, that one has only to focus something on the plate, keep it there till it has had sufficient exposure, put it into a developer, fix it, and the thing is done. Without some such explanation, it would have been necessary to detail my own work in Switzerland at considerable length; and, besides, one is naturally inclined to ask—If the absorbing medium is not essential to the process, why was not the corona photographed before? I venture to think that, had an astronomer and a photographer worked together, they would not have done it, and that Dr. Huggins accomplished it only because he was both.

Dr. Huggins was only able to get the corona on very rare occasions in England, the atmospheric glare being usually too great. Not only is the air clearer in Switzerland generally, but I was sent to the Riffel, and had to erect my observatory at an elevation of about 8500 feet. The instrument used was constructed by Grubb, and had a reflector of speculum-metal of three inches working aperture and about six feet focus. The total length of the instrument was nearly twelve feet, the light being allowed to fall down a tube containing a number of diaphragms placed a few inches apart, the Sun being brought to a focus near the centre of the instrument, where a rapidly moving shutter passed close to and in front of the sensitive plate. The whole was well blacked within and swathed without in flannel and swans'-down calico to prevent any convection-currents inside. A cap was fitted on the front of the tube, and was only removed when an exposure was about to be made. Several kinds of plates were used, chloride of silver giving the best results, the maximum point of sensitiveness of this compound being in the ultra-violet. Bromide of silver, which has its maximum about G, came next in the quality of results, whilst on iodide of silver (maximum below G) little or nothing could be obtained. The corona therefore appears to be richer and better able to overcome the glare of the atmosphere in the violet; but the greater facility for obtaining contrast with chloride-plates has also to be taken into account.

The chief difficulties I experienced at first were purely mechanical, and were mainly due to the shutter, which, passing across the Sun's image, gave rise to false detail by reflection from the edges of the flaps, when it was not kept well blacked. When the disk was tried, therefore, it was so mounted that the shutter passed between the disk and the sensitive film. It was, moreover, a little larger than the Sun's image and was well blacked, so that there could neither be diffraction from its edges nor any appreciable reflection towards the mirror, and in addition it rendered halation impossible. The results so far may be thus summed up:—

1. As would be expected, the results are better than had been obtained in England, in spite of the red haze which has been always present round the Sun, and which visitors to Switzerland have commented on in several of the scientific journals recently.

2. Results on the same day are almost, if not quite, alike, both with disk and without.

3. The corona varies more or less from day to day.

4. The clearer the day the better the results.

5. The series extends over a period of two months, one month's results being free from effects that require elimination.

C. Ray Woods.

CORRESPONDENCE.

To the Editor of 'The Observatory.'

The first recorded English Eclipse.

Sir,—

It cannot be a matter devoid of astronomical curiosity to notice which was the first eclipse that is recorded as having been seen in our own land. None of the Moon is mentioned until the year A.D. 734. But for the first one of the Sun we may go back two centuries earlier. The 'Chronicon Scotorum' gives an eclipse for Ireland in the year 493; but as none in that year or in the previous and subsequent ones will answer, it is evident there is some error. In 538, the 'Saxon Chronicle' records that "the Sun was eclipsed fourteen days before the Calends of March from early morning till nine." Calculation affords an eclipse that will just answer to the circumstances.

In 540, however, another solar eclipse is mentioned in these words in the 'Saxon Chronicle'—"In this year the Sun was eclipsed on the 12th of the Calends of July, and the stars appeared full nigh half an hour after nine;" or, to use the words of the Chronicle of Holyrood—"A.D. 540. The Sun was once more eclipsed on the twelfth of the Calends of July, and for half an hour the stars were visible from the third hour of the day till nearly the sixth." This must have been in the days of King Arthur, and two years before he died at Glastonbury. It seems to have been a fruitful year in celestial sights, for Matthew of Westminster informs us that "real blood rained out of the clouds," probably signifying a red aurora, and so the first recorded aurora in our land. The year was also marked by the appearance of a bright comet. It will be noticed that more than a mere record of the eclipse is given. It seems to have attracted much attention. The phrase "the stars appeared" gives the same difficulty as is involved in taking the eclipse of B.C. 431, as that in which Thucydides says "some of the stars shone out," for the eclipse of A.D. 540 covered barely two-thirds of the Sun's diameter in England. It is hardly likely that the account of the appearance of stars refers to something seen in other lands. Again the eclipses of 1860 and 1870, the last large ones in our land, were not sufficient to bring out stars, though five-sixths of the Sun's disk were obscured on each occasion. One is led therefore to search other years; and in 536 there was an annular eclipse of the Sun, in the middle of the day, on September 1. The magnitude of this eclipse satisfies the record far better than that of 540. The time given as "half an hour after nine" is little to be regarded, for the same Chronicle in describing the eclipse of 1140 says it was "near the noon tide of the day when men were eating," and calculation shows that it was about 3^h P.M. If this conjecture be correct, then 536 marks the year of the first recorded eclipse in this country.

Faithfully yours,

S. J. Johnson.

Melplash Vicarage, Bridport,
1884, Nov. 4.

Fireball in Twilight.

SIR,—

This evening at $5^h\ 28\frac{1}{4}^m$ I was fortunate enough to witness the appearance of a very fine meteor one third of the diameter of the Moon, intensely white in colour, tinted with green at the edge. Course from about 10° below ζ Ursæ Majoris almost to a point 5° below Arcturus. It must have passed across 12 Can. Venat., a star then invisible from twilight. Duration four seconds; slightly pear-shaped, with momentary train. It disappeared and reappeared once. Seen against a dark sky, the illumination would have been superb.

Faithfully yours,

S. J. JOHNSON.

Melplash Vicarage, Bridport,
1884, Nov. 3.

Alleged Eclipse at the Death of Nerva.

SIR,—

Although doubtless many of your readers are following with interest Herr F. K. Ginzel's investigations on eclipses, perhaps a few words may be acceptable concerning that which is stated to have occurred at the time of the death of the Emperor Nerva, if only on account of the decisive evidence it affords that we must be cautious in assuming, when we read in ancient authors of an *ἔκλειψις ἡλίου*, or "defectus solis," that an eclipse of the Sun is necessarily to be understood as having taken place.

The Pseudo Aurelius Victor, speaking of the death of Nerva, says:—"eoque die quo interiit, solis defectus factus est."

Now Nerva died, in all probability, towards the end of January in A.D. 98, and no eclipse could have taken place that month. During the year two solar eclipses took place, of which the one (on the 21st of March) Herr Ginzel finds was only central in North America, Iceland, and (towards sunset) on the coast of Norway, so that it could scarcely have been noticed in Central, still less in Southern Europe; whilst the other, on the 13th of September, passed only over the great Southern Ocean.

Nor should we mend matters much by supposing that there is a mistake in the historic date of the death of Nerva. In A.D. 97 no considerable eclipse of the Sun took place, and in A.D. 96, though there were two, they were both visible only in America.

Seyffarth contends that Domitian was assassinated in the year 97, and that the death of Nerva, 16 months afterwards, occurred in the year 99, not 98 as is generally supposed. He then endeavours to identify the eclipse with one which took place on the 3rd of September in the former of those years. But Herr Ginzel shows that the line of centrality of that eclipse passed over Central Africa and the Indian Ocean, and that the effect at Rome was even more trifling than in the case of that of March 21st, A.D. 98. Another total eclipse of the Sun took place indeed earlier in the year 99, on the 10th of March, but it was not visible in any part of Europe.

Whilst once more on the subject of eclipses, I cannot refrain from expressing my concurrence with Prof. Newcomb in doubting very much whether the phenomenon stated by Xenophon ('Anabasis,' iii. 4) to have taken place when the Persians took Larissa (the modern Nimrûd) from the Medes, was really an eclipse of the Sun as has been supposed, and for which the date B.C. 557, May 19th, has been assigned.

Yours faithfully,

W. T. LYNN.

Blackheath, 1884, Nov. 4.

NOTES.

THE CAPE CATALOGUE FOR 1850.—A catalogue of 4810 stars for the epoch 1850 has just been issued by H.M. Stationery Office. This catalogue has been compiled from observations made at the Cape Observatory during the years 1849 to 1852 under the direction of the late Sir Thomas Maclear. The necessary reductions were pushed on by Mr. Stone during his years of office at the Cape, and the work has been finished and published by his successor, Dr. Gill. The right-ascension observations contained in the catalogue were made with Dollond's Transit, which was mounted by Fallows in 1828; and the North Polar Distances were observed with Jones's Mural Circle, mounted in 1839. From a comparison of the star-places in the Catalogue with those in the Cape Catalogue for 1880, it appears that the agreement in N.P.D. is satisfactory, there being a practically uniform difference (within the limits of error) of most of the separate N.P.D.'s between the catalogues through the range of polar distance over which the observations extend. But the comparison in right ascension is by no means so satisfactory, as there are considerable discordances, depending not only on R.A., but also on polar distance, which Dr. Gill hints may arise from the non-reversible character of the Cape Transit-Circle, as it is possible either that the collimation of the instrument may not remain constant at different altitudes, or, less probably, that the observations may be affected by lateral refraction, caused by the non-symmetrical position of the wings of the building with respect to the instruments. At all events, Dr. Gill finds that there is more marked difference depending on N.P.D. between the R.A.'s of the Cape and Greenwich Catalogues for 1860 (when non-reversible instruments were employed) than in the 1840 catalogues, when reversible transits were employed at both observatories. And a comparison of the R.A.'s of the Cape and Greenwich Catalogues for 1850 appears to bring additional evidence to show that the R.A.'s of the Cape Catalogue for 1850 may be accepted with considerable confidence. This, of course, raises the very important question as to what is the cause of the discordance between the R.A.'s of the Cape Catalogues for 1850 and 1880, a matter which doubtless Dr. Gill will investigate further. A. M. W. D.

THE PRIME MERIDIAN CONFERENCE.—The Washington Prime Meridian Conference closed on November 1. On the question of reckoning longitude, the Conference reversed the decision of the Geodetic Conference held at Rome a year ago, and resolved that it should be counted from Greenwich in *both* directions, and therefore only up to 180°, longitude east to be *plus*, and longitude west to be *minus*. The hours of the universal day, which is to commence at Greenwich mean midnight, are to be counted from 0^h up to 24^h. As the astronomical day now commences with mean noon, a change will have to be made in this respect; this the Astronomer Royal proposes to make, so far as regards the Royal Observatory, on the first of January next, from which date the Greenwich astronomical and civil days will be the same.

THE TOTAL LUNAR ECLIPSE OF 1884, OCT. 4.—The occultations ascribed to the Radcliffe Observatory in the table given on p. 338 of the November number of the 'Observatory' were not observed there, but at the other Oxford Observatory—the Oxford University Observatory, under the direction of Prof. Pritchard.

NEW SOUTHERN DOUBLE STARS.—Mr. H. C. Russell, the Government Astronomer at Sydney, has recently published a list of 146 double stars discovered by himself and his assistant Mr. Hargrave at the Sydney Observatory.

α CENTAURI.—With reference to the recent measures of this celebrated binary given by Mr. Tebbutt in the October number of the 'Observatory,' the following comparison with the position-angle and distance computed from Mr. Downing's elements ('Monthly Notices,' 1884, March) may be of interest, the computation being made for the mean date of Mr. Tebbutt's observations, viz. 1884·553, and the simple mean of the measures being taken:—

Computed position-angle 200°·3. Computed distance 12″·71.
Observed position-angle 199°·8. Observed distance 12″·93.

THE last day for depositing chronometers at the Royal Observatory, Greenwich, for the forthcoming Annual Trial, will be Monday January 5, 1885. Chronometers for trial may be deposited during any morning of the preceding week.

Application for permission to send chronometers for trial must be made to the Hydrographer, Admiralty, London, S.W., not later than December 20.

A NEW Minor Planet, No. 245, magnitude 11·5, was discovered on 1884, Oct. 27, by Herr Palisa at Vienna. Minor Planet No. 240 has received the name of Vanadis.

MR. GRUBB's paper on "The Adjustment of Equatoreal Telescopes" will appear in the number of the 'Observatory' for January next.

Maxima and Minima of Variable Stars in 1884, December.

M, signifies maximum ; *m*, minimum.

Dec. 2·4 ζ Geminorum, M.
2·6 W Virginis, M.
4 S Tauri, M.
4·5 T Monocerotis, *m*.
6 R Coronæ, *m*.
6 R Cancri, M.
7·5 ζ Geminorum, *m*.
11·7 W Virginis, *m*.
12 R Aurigæ, *m*.
12 S Pegasi, M.
12 T Monocerotis, M.
12·6 ζ Geminorum, M.
14 U Monocerotis, M.
17 S Leonis, M.
17 S Cygni, M.

Dec. 17·7 ζ Geminorum, *m*.
19·9 W Virginis, M.
20 R Canis Minoris, M.
22 S Vulpeculæ, M.
22 U Capricorni, M.
22·7 ζ Geminorum, M.
23 R Lyræ, *m*.
23 W Scorpii, M.
26 S Aquilæ, *m*.
27·8 ζ Geminorum, *m*.
28 S Ursæ Majoris, *m*.
29 T Cassiopeiæ, *m*.
29 R Ceti, M.
31 V Virginis, M.
31·5 T Monocerotis, *m*.

Variables of Short Period.

δ Cephei.

	d.
Dec.	1·25, M.
	6·65, M.
	10·45, *m*.
	15·75, *m*.
	17·35, M.
	21·15, *m*.
	22·75, M.
	26·55, *m*.
	28·15, M.
	31·85, *m*.

W Sagittarii.

	d.
Dec.	2·6, *m*
	5·8, M.

X Sagittarii.

	d.
Dec.	1·7, *m*.
	4·6, M.
	8·7, *m*.
	11·6, M.

η Aquilæ.

	d.
Dec.	3·4, *m*.
	5·8, M.
	10·6, *m*.
	17·8, *m*.
	20·2, M.
	27·3, M.

U Cephei, *m*.

		h	m
Dec.	2	4	38
	4	16	28
	9	16	7
	14	15	47
	19	15	26
	24	15	6
	29	14	45

Algol, *m*.

		h	m
Dec.	2	10	39
	5	7	28
	16	18	44
	19	15	32
	22	12	21
	25	9	10
	28	5	59

λ Tauri, *m*.

		h	m
Dec.	2	15	58
	6	14	50
	10	13	42
	14	12	34
	18	11	27
	22	10	19
	26	9	11
	30	8	4

S Cancri, *m*.

		h	m
Dec.	6	19	20
	16	6	58
	25	18	35

δ Libræ, *m*.

		h	m
Dec.	3	6	55
	5	14	46
	10	6	29
	12	14	20
	17	6	3
	19	13	54
	24	5	37
	26	13	28
	31	5	11

U Coronæ, *m*.

		h	m
Dec.	1	4	37
	4	15	29
	11	13	11
	18	10	54
	25	8	36
	28	19	27

Ephemeris for Encke's Comet.
By Dr. O. Backlund.

Berlin Midnight.	R.A. h m s	Dec. ° ′ ″	Berlin Midnight.	R.A. h m s	Dec. ° ′ ″
Dec. 1......	22 43 42	+4 19 32	Dec. 17......	22 45 59	+3 41 21
3......	22 43 29	+4 11 22	19......	22 46 52	+3 40 49
5......	22 43 24	+4 4 12	21......	22 47 54	+3 41 11
7......	22 43 29	+3 58 0	23......	22 49 3	+3 42 25
9......	22 43 42	+3 52 46	25......	22 50 18	+3 44 31
11......	22 44 4	+3 48 30	27......	22 51 41	+3 47 27
13......	22 44 34	+3 45 11	29......	22 53 11	+3 51 13
15......	22 45 12	+3 42 48	31......	22 54 47	+3 55 46

Ephemeris for Comet 1884 c (Wolf).*
By Dr. A. Krueger.

Berlin Midnight.	R.A. h m s	Dec. ° ′	Berlin Midnight.	R.A. h m s	Dec. ° ′
Dec. 2......	23 25 9	−4 59·8	Dec. 12......	23 50 59	−5 57·9
4......	23 30 18	−5 14·8	14......	23 56 10	−6 4·7
6......	23 35 28	−5 28·0	16......	0 1 21	−6 10·1
8......	23 40 38	−5 39·6	18......	0 6 32	−6 14·0
10......	23 45 48	−5 49·5	20......	0 11 43	−6 16·6

Differences of Right Ascension and Declination between Iapetus and the Centre of Saturn. By A. Marth †.

Iapetus − Saturn.

Greenwich Noon.	α−A. s	δ−D. ″	Greenwich Noon.	α−A. s	δ−D. ″
Dec. 1....	− 6·8	+134	Dec. 17....	−41·4	−114
2....	10·1	121	18....	41·5	127
3....	13·5	108	19....	41·2	140
4....	16·7	93	20....	40·7	152
5....	19·8	78	21....	39·9	162
6....	22·8	63	22....	38·8	172
7....	25·6	47	23....	37·4	180
8....	28·3	30	24....	35·7	187
9....	30·7	+ 14	25....	33·9	192
10....	33·0	− 3	26....	31·7	197
11....	35·0	20	27....	29·4	200
12....	36·7	36	28....	26·8	201
13....	38·2	53	29....	24·1	201
14....	39·4	69	30....	21·2	200
15....	40·4	84	31....	18·2	197
16....	−41·0	− 99	32....	−15·0	−193

* Astron. Nachr. No. 2622.
† Monthly Notices, Vol. xliv. No. 9.

Greenwich Mean Times of the East Elongations of the Seven Inner Satellites of Saturn.

Note.—In the case of Mimas, which can only be seen at elongation, the times of those elongations, both East and West, which are visible at Greenwich are given.

Mimas (East).			Enceladus.			Tethys.			Dione.		
		h			h			h			h
Dec.	5	14·4	Dec.	2	3·0	Dec.	1	8·6	Dec.	2	6·1
	6	13·0		3	11·9		3	5·9		4	23·7
	7	11·7		4	20·8		5	3·1		7	17·4
	8	10·3		6	5·7		7	0·4		10	11·0
	9	8·9		7	14·5		8	21·7		13	4·7
	10	7·5		8	23·4		10	19·0		15	22·3
	11	6·1		10	8·3		12	16·3		18	15·9
	21	14·8		11	17·1		14	13·6		21	9·5
	22	13·4		13	2·0		16	10·9		24	3·2
	23	12·0		14	10·9		18	8·2		26	20·9
	24	10·6		15	19·8		20	5·5		29	14·6
	25	9·2		17	4·7		22	2·7			
	26	7·8		18	13·5		24	0·0			
	27	6·5		19	22·4		25	21·2	Rhea.		
				21	7·3		27	18·5	Dec.	3	2·7
Mimas (West).				22	16·1		29	15·8		7	15·0
Dec.	12	16·0		24	1·0		31	13·1		12	3·2
	13	14·6		25	9·9					16	15·5
	14	13·2		26	18·8					21	3·9
	15	11·8		28	3·7	Titan.				25	16·1
	16	10·4		29	12·6	Dec.	3	7·1		30	4·4
	17	9·1		30	21·5		19	3·4		34	16·7
	18	7·7									
	19	6·3									
	29	15·0	Hyperion.								
	30	13·6	Nov.	29	6·9						
	31	12·2	Dec.	20	10·0						

The following table will furnish the means for inferring roughly the distances and position-angles of the satellites from the diagram given on page 272.

		Position-angle of Minor Axis of Ring.			Semi-major axis of orbit of Titan.
Nov.	29	354° 29′	Nov.	29	208″·9
Dec.	9	354 34	Dec.	9	209 ·6
	19	354 39		19	209 ·4
	29	354 45		29	208 ·4

Astronomical Memoranda, 1884, *December.*

Equation of Time:—Sun *before* Clock, Dec. 1, 10^m 33^s; Dec. 11, 6^m 17^s; Dec. 21, 1^m 23^s; Dec. 31 (*after*), 3^m 32^s.

Sidereal Time at Mean Noon:—Dec. 1, 16^h 42^m 59^s; Dec. 11, 17^h 22^m 25^s; Dec. 21, 18^h 1^m 50^s; Dec. 31, 18^h 41^m 16^s.

Sun.	Rises.	Sets.	Position-angle of axis.	Heliogr. co-ordinates of centre of disk. Lat.	Long.
	h m	h m	° ′	° ′	° ′
Dec. 2	19 48	3 52	15 22	+0 24	12 25
7	19 55	3 50	13 18	−0 14	306 31
12	20 0	3 50	11 6	0 52	240 39
17	20 4	3 51	8 49	1 30	174 46
22	20 7	3 53	6 27	2 8	108 54
1885. 27	20 8	3 57	4 2	2 44	43 3
Jan. 1	20 8	4 1	1 35	−3 19	337 12

The position-angles of the Sun's axis and the co-ordinates of the centre of the disk are given for Greenwich Mean Noon.

Moon.	*sets.*		*rises.*		*sets.*
	h m		h m		h m
Dec. 1	18 50	Dec. 11	14 34	Dec. 21	8 8
2	*rises*	12	15 38	22	9 13
3	5 11	13	16 40	23	10 19
4	6 17	14	17 40	24	11 27
5	7 29	15	18 36	25	12 38
6	8 44	16	*sets*	26	13 51
7	9 58	17	4 21	27	15 6
8	11 10	18	5 11	28	16 21
9	12 20	19	6 6	29	17 35
10	13 28	20	7 5	30	18 43
				31	

Full Moon, Dec. 2, $7^h\ 0^m$; Last Quarter, Dec. 8, $23^h\ 31^m$; New Moon, Dec. 17, $1^h\ 25^m$; First Quarter, Dec. 25, $1^h\ 21^m$; Full Moon, Dec. 31, $17^h\ 26^m$.

Mercury in Sagittarius, at greatest elongation (20° 8′ E.) Dec. $17^d\ 14^h$, stationary Dec. $25^d\ 1^h$.

Venus a morning star, in conjunction with the Moon Dec. $13^d\ 22^h$. Diameter:—Dec. 1, 13″·7; Dec. 31, 12″·1. Illuminated portion of disk 0·826 on Dec. 15.

Dec. 1, R.A. $14^h\ 6^m{\cdot}8$, Dec. 10° 42′ S., tr. $21^h\ 24^m$, rises $16^h\ 17^m$
31, 16 34 ·8, 20 39 S., 21 55 17 47

Mars an evening star, in conjunction with the Moon Dec. $18^d\ 7^h$. Diameter:—Dec. 1, 4″·8; Dec. 31, 4″·8. Illuminated portion of disk 0·993 on Dec. 15.

Dec. 1, R.A. $17^h\ 46^m{\cdot}7$, Dec. 24° 17′ S., tr. $1^h\ 4^m$, sets $4^h\ 51^m$
31, 19 27 ·1, 22 58 S., 0 46 4 42

Jupiter in Leo, stationary Dec. $21^d\ 6^h$. Diameter:—Dec. 1, 35″·5; Dec. 31, 38″·9.

Dec. 1, R.A. $10^h\ 29^m{\cdot}6$, Dec. 10° 30′ N., tr. $17^h\ 44^m$, rises $10^h\ 47^m$
31, 10 31 ·5, 10 27 N., 15 48 8 50

Saturn, in opposition to the Sun Dec. $11^d\ 19^h$.

Dec. 1, R.A. $5^h\ 24^m{\cdot}0$, Dec. 21° 42′ N., tr. $12^h\ 39^m$, rises $4^h\ 35^m$
31, 5 13 ·6, 21 35 N., 10 30 sets 18 35

	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Inner Ring. Maj. Axis.	Inner Ring. Min. Axis.	Ball. Diam.
. 19......	46″·54	20″·90	30″·95	13″·90	18″·6

he south side of the rings is visible, the elevation of the Earth e their plane on Dec. 19 being 26° 41′ S., and of the Sun 49′ S.

Uranus, in quadrature Dec. 23^d 18^h.

c. 1, R.A. 12^h 9^m·7,	Dec. 0° 15′ S.,	tr. 19^h 23^m,	rises 13^h 18^m
31,	12 11 ·9,	0 28 S.,	17 28	11 22

Neptune.

ec. 1, R.A.	3^h 18^m·0,	Dec. 16° 23′ N.,	tr. 10^h 33^m,	rises 3^h 3^m
31,	3 15 ·3,	16 14 N.,	8 33	sets 16 4

Phenomena.

	G. M. T. h m			G. M. T. h m	
Dec. 1	12 44	J. ii. Ec. D.	Dec. 17	15 2	J. ii. Tr. I.
	16 3	J. i. Ec. D.		17 45	J. i. Oc. R.
	18 11	J. ii. Oc. R.		17 52	J. iii. Tr. E.
2	14 28	J. i. Tr. I.		17 57	J. ii. Tr. E.
	16 47	J. i. Tr. E.	18	12 38	J. i. Tr. I.
3	10 51	B.A.C. 1930 Oc. R. 335°.		14 57	J. i. Tr. E.
	13 1	J. ii. Tr. E.	19	12 12	J. i. Oc. R.
	14 4	J. i. Oc. R.		12 29	J. ii. Oc. R.
4	15 35	λ Geminorum Oc. D. 84°.	23	7 52	B.A.C. 7986 Oc. D. 102°.
	16 44	λ Geminorum Oc. R. 254°.	24	16 10	J. i. Ec. D.
				17 27	J. ii. Tr. I.
				17 57	J. iii. Tr. I.
6	12 14	B.A.C. 3122 Oc. R. 201°.	25	14 27	J. i. Tr. I.
				16 47	J. i. Tr. E.
	15 43	J. iii. Ec. D.	26	10 39	J. i. Ec. D.
7	10 16	π Leonis Oc. R. 212°.		14 1	J. i. Oc. R.
				14 57	J. ii. Oc. R.
8	15 19	J. ii. Ec. D.	27	11 14	J. i. Tr. E.
	17 56	J. i. Ec. D.	28	11 26	J. iii. Oc. R.
9	16 20	J. i. Tr. I.	29	8 34	63 Tauri Oc. D. 9
	18 39	J. i. Tr. E.			
10	12 24	J. i. Ec. D.		8 40	B.A.C. 135 Oc. D.
	12 34	J. ii. Tr. I.			
	14 6	J. iii. Tr. E.	30	8 27	115 Tauri Oc. D. 1
	15 30	J. ii. Tr. E.			
	15 55	J. i. Oc. R.		17 34	130 Tauri Oc. D.
11	13 7	J. i. Tr. E.			
14	15 32	J. iv. Oc. D.	31	12 29	J. iv. Oc.
15	17 55	J. ii. Ec. D.		13 33	26 Gemin Oc. D.
17	14 16	J. iii. Tr. I.			
	14 17	J. i. Ec. D.		18 4	J. i. Ec.

The angles are reckoned from the *apparent* N. point towa right of the Moon's inverted image. EDI

ERRATUM IN No. 91.
Page 341, line 24, *for* Longitude *read* Time.

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